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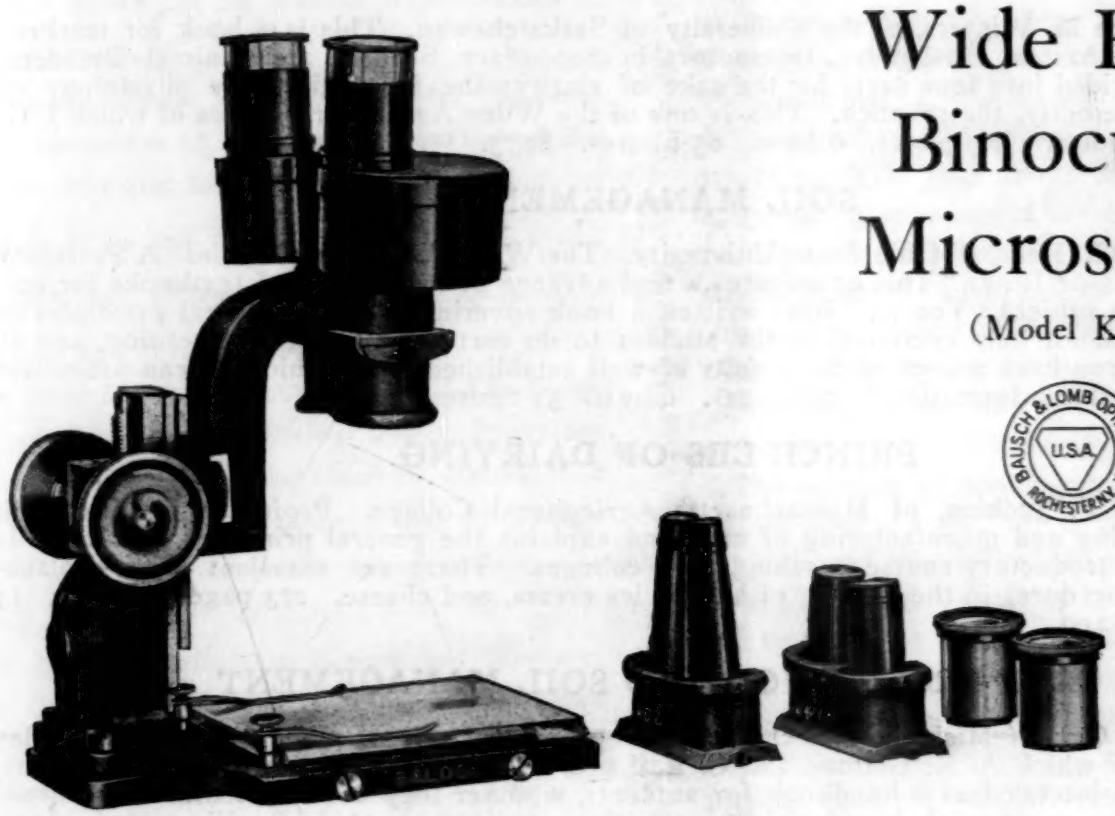

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VOL. LXI MAY 15, 1925 No. 1585

CONTENTS

The Opportunity of Anatomy: PROFESSOR FLORENCE R. SABIN 499

Restoration of the Oldest Known Forest: J. AUSTEN BANCROFT 507

John Van Denburgh: DR. BARTON W. EVERMANN 508

Scientific Events:

Moseleyum and the Names of Elements: The American Association of Museums; Pharmaceutical Research; National Research Fellowships in Medicine 510

Scientific Notes and News 512

University and Educational Notes 515

Discussion and Correspondence:

The Ether Drift: DR. PAUL R. HEYL. *The Disappearance of Helium in Geissler Tubes:* PROFESSOR HARVEY B. LEMON. *The Classified Continuation Card Catalogue of the Bibliography of Fishes:* E. W. GUDGER 515

Special Articles:

The Life of Metastable Helium and Mercury: DR. CARL ECKART. *The Successive Stimulation of the Arc Lines of Helium below the Ionization Potential:* DR. C. B. BAZZONI AND J. T. LAY 517

The National Academy of Sciences:

Abstracts of papers presented at the Washington Meeting 518

Science Service x

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THE OPPORTUNITY OF ANATOMY[1]

IT has been the custom of the past few years for the person whom you have chosen as your presiding officer to give in this hour some comprehensive survey of his own research. But last year you gave a generous allotment of your time to a symposium on the subject in which I am especially interested. At that time I presented the work of my colleagues and myself, it is true in an incomplete form, but as our experiments have since been analyzed and published, I propose, with your permission, to speak on a more general topic. I wish to return to an earlier custom of this society and consider the general subject of teaching. This year marks the end of my career as a teacher: I have taught my last class, I have ceased to be a professional teacher, but remain a professional student; but I have taught for twenty-six years, twenty-three years in a medical school, with such pleasure that I wish to record some of the changes which I have personally lived through in the teaching of anatomy.

The relation of anatomy to medical research is to be my special topic, for both in its position at the beginning of the medical course and in the nature of its subject-matter, anatomy has facilities unsurpassed for turning the minds of students toward research. From the programs of our meetings it is clear that gross anatomy is enlarging its field for research into the domain of anthropology, that neurology is at the present time one of the most brilliant subjects for research, in its almost untrodden field for the correlation of structure and function, but it is specifically of histology that I wish to speak this morning, to the thesis, that, in histology, one has a rare opportunity to teach knowledge in its growing zone. That is the definition with which Havelock Ellis in his most fascinating new book, "The Dance of Life," has illuminated the relation of science to modern education.

When I began the teaching of histology, twenty-three years ago, the subject was a minor discipline. Histology began with the discovery of the cell, and through its early period it was the isolated cell that dominated its subject-matter; but the cell, isolated by methods of maceration, dilute alcohol for the epithelial cells and weak acids for muscle, had been dead long before it was studied and so we had only general concepts concerning its form and its relation-

[1] Presidential address given before the American Association of Anatomists, 1925.

ships. The microtome was then introduced and we began to study sections. That marked the next phase of the study of histology, namely, the survey of all the organs of the body, in order to learn the general topography of the tissues. It was during this phase of the subject that text-books began to appear. When I began teaching histology we had these two techniques. We gave quite simple exercises with isolated cells of each type and about three sections each day to be stained, mounted and studied by the student. We had also frozen sections which we studied mainly for the fibers of the connective tissues. We used them, here and there, to analyze the structure of an organ; for instance, following the work of His, we shook frozen sections of lymph glands in a test tube and speculated on why the follicles fell out and the rest of the framework remained. When I took charge of our course in histology, Dr. Mall gave me only one direction, but advised me to read two books, both of them by English teachers—one, Beale's "How to Work with the Microscope,"[2] and the other a little book by Miall on "Thirty Years of Teaching."[3] The book of Beale is not a cut-and-dried text-book, with a mere statement of facts, but is the record of a man who had used a microscope with joy to find out things concerning which he was intensely curious.

Miall's book has charm. He was a teacher of biology in the Yorkshire College and his concept of teaching was to help students to love their work. To quote, "Lecturing aims at giving information, teaching aims at discipline. To both teacher and student the way in which a result is got may be far more important than the result itself. . . . When two biologists meet, they tell one another things, but they don't offer discipline to one another. The professor and an advanced class should be on something like the same terms." Or again, "The spirit of inquiry is only to be communicated by those who have it, who habitually inquire themselves."

The specific counsel which Dr. Mall gave me was that in outlining any plan of work for students one should never make the directions so specific as to rob the student of his pleasure in discovery and make him mechanically follow a definite procedure. This is the flaw in most of the formally published courses, that they tend to destroy for the student all the pleasure of personal discovery. It is clear that the nature of instructions to be given to students must vary to some extent, indeed perhaps to a marked extent, with the nature of the material to be handled. If material is rare or fragile or involves the use of anesthetized animals, then the directions must be specific, but there are certainly two subjects in the premedical sciences, namely, histology and pathology, where material can be so unlimited that the student can be given great liberty and be taught wholly in the spirit of finding things out for himself.

I have said enough to make it clear that it is of histology in its growing zone that I wish to speak. We have passed through two phases of histology, through the primary analysis of its elements and of how they are put together to form organs and we have now a third and entirely new type of subject-matter. There is, it seems to me, no longer any need of an elementary course in histology, in a medical school for which the entering students have been adequately prepared in biology and in the use of the microscope. I am well aware that many of you may not agree with this idea, but it is my real conviction. How else shall we progress in education unless we condense those things which have become so well known that they have been simplified and teach knowledge in its growing zone. By an elementary course in histology, I mean one in which the major part of the work consists in learning to describe and identify sections; this is the finished part of our subject, not its zone of growth. The subject-matter of this part is simple, is well worked out in text-books and can be readily mastered by the student without much aid. It seems to me that, in each subject taught to advanced students, the teacher should make a clear analysis in his own mind of that part of the subject-matter which is static, well known, well presented in text-books, and hence readily available to the students' own initiative, and that part which is dynamic, concerning which there are marked differences in opinion by experts, and concerning which he may confidently expect development in the coming years. There are assuredly dangers involved in regarding any body of scientific knowledge as static, but at the risk of wearying you I wish to make quite specific the type of material which seems to me feasible subject-matter for a course in histology at the present time, and I will take four examples from my own experience. In presenting these examples, I have also in mind that when a certain foreign commission visited our laboratory to study our methods of teaching, it was clearly in the minds of our guests that, without the specific material of modern biology, histology was lacking in adequate subject-matter for medical students. The frank discussion of such differences in plans for education I consider of value to teachers; it is obvious that we would give modern biology as the preparation for histology.

My first example will be the spleen. In the first place, I would give to each student a section of human spleen and of the spleen of one or two animal types

[2] Harrison, London, 1868.

[3] L. C. Miall, professor of biology in the Yorkshire College, Macmillan and Co., London, 1897.

in advance. It has been our custom to give the sections to be stained and mounted by the students at the close of the period before the subject is taken up in class; that assures ample material in the way of sections to be used for reference. For the work on the spleen the student would have, in his own collection, not only sections of the spleen but also of hemal glands and of lymph glands. Moreover, one of his sections would be a mesenteric lymph gland in which he had filled the lymphatic sinuses with ink by injecting the lymphatic vessels of the intestine. The specific work on the spleen would then be directed toward three ideas—what is the nature of the circulation in the spleen, what kind of endothelium lines its veins and what are the free cells in its pulp. Not one of these problems can be analyzed without experiment. I should begin with Mall's crucial experiment to understand the remarkable nature of the splenic circulation. To prepare the material, take a dog the day before the class exercise and tie off all the splenic veins, slip the organ back into the abdominal cavity for half an hour, at the end of which time the animal will have bled into its own spleen until the organ is twice its normal size. Then tie off the arteries and fix the entire organ in formalin without any rupture of its capsule. A second similar experiment, in which all the vessels are cut before fixation brings the organ back to the normal size, very quickly makes a convincing comparison. The class then would have frozen sections made from the two experiments; in one, both the pulp and the veins of the pulp will be engorged with blood; in the other both will be empty. This, then, would give the material from which to discuss how the splenic pulp can be so completely filled and then so quickly emptied. In a lecture one could then show why the question of whether there were specialized channels in the pulp between the arteries and the veins of the pulp dominated research on the spleen so long and how the work of Weidenreich and Mall had finally demonstrated that it is the splenic pulp as a whole that intervenes between artery and vein. In the frozen sections of such material there will be occasional places in which the blood has run out of a vein of the pulp, and such places can be used to illustrate the contrast between the pulp and its veins; but for this purpose we have in Baltimore some beautiful complete injections of the veins of the splenic pulp, made by Dr. Mall and never illustrated in his publication. In these preparations he succeeded in filling the pulp so completely with one substance and the veins with another that both are quite distinct.

Our next topic would be the nature of the endothelium lining the ampullae and the veins of the pulp; and here we would use the technique introduced by Mollier, who has shown that, when the spleen is fixed after a moderate distention produced by circulating the fixing fluid through the organ and the sections stained with an intense protoplasmic dye, such as acid fuchsin, the endothelial lining of the veins can be seen to be riddled with holes. Each student would receive such a section for his collection and could then find for himself the peculiar, widely fenestrated endothelium of the veins of the splenic pulp and could then more easily understand from such evidence how the animal can bleed into the splenic pulp and then empty the pulp so completely. We would give out one section in which a very little ink or Prussian blue had been injected in one quick spurt into the splenic artery. Such an injection of the splenic arteries represents an extravasation, if you will, into the splenic pulp, or in other words shows that the splenic pulp is in lieu of a capillary bed. These points we regard as feasible to demonstrate and that after such a presentation of the nature of the entirely unique circulation of the spleen, an organ in which there is nothing but a framework and a vascular bed, the student could take an ordinary section of the spleen, find its trabeculae and think of the meaning of its bands of muscle, identify the Malphigian corpuscles and see for himself why it is that the relation of the splenic pulp to the veins of the pulp can not be analyzed from the study of normal sections alone.

We would give one more preparation of the spleen, perhaps one of the most instructive, namely, a section of a spleen from a rabbit which had received repeated injections of trypan blue, or better of carmine, until the veins of the splenic pulp had become so clogged with cells, in our view, endothelial phagocytes or clasmotocytes, that the veins stand out in a section even when seen with the low power of a microscope. Here we would help the student to follow the endothelium of the sinuses and to see the clasmatocytes both within the sinuses and free in the pulp and then give him a chance to make his own judgment as to whether Aschoff's term of "reticulo-endothelial apparatus" is significant or confusing. We believe that reticulum and the free endothelial derivatives are both quite distinct in the pulp, but the student will surely find that the concept of a reticulo-endothelial apparatus so dominates opinion in pathology to-day that he may well spend much time in studying this section and getting his own ideas on this subject.

The second topic which I wish to present is the nature of the cells of the connective tissues as they can be analyzed by the vital technique. Here I should begin with the experiment of Professor Maximow in the analysis of the fibroblasts from the clasmatocytes of the diffuse connective tissues by the use of vital neutral red. In the work which Dr. Cunningham, Dr. Doan and I have just published on the cells of the

connective tissues, we have given a technique for studying the living cells of the splenic pulp. If one takes a finely drawn out glass pipette and plunges it into the spleen of a rabbit while the circulation is active, blood and free cells will run up into the pipette and one can then make a preparation entirely comparable to a blood film. These are made either on a coverslip for fixing and staining with the ordinary technique, or on a slide which has been coated with a film of vital neutral red and Janus green combined. In such a preparation from the spleen the cells will differ according to whether the pipette entered the pulp or a follicle. If the specimen comes from a follicle, lymphocytes will predominate, but if from the pulp, then there will be two predominating types of cells, the large free clasmatocytes, filled with red blood-cells or their débris, and the monocytes with their beautifully stained rosettes and outlying mitochondria. This is not an easy technique to give a large class, because the spleen will not stand an unlimited number of such punctures, but one animal may be used for about fifteen students. These two types of cells, the clasmatocyte and monocyte, can perhaps be most easily demonstrated to a large class by making a vital film from a scraping of a freshly cut surface of the lung. For this experiment we should inject a little ink with a hypodermic needle into the trachea of a guinea pig the day before, following the work of Dr. Wislocki, and from this material we should find no difficulty in securing for every student a successful preparation. I may say in passing that the method of puncture entirely solves the question of giving a class good films of bone marrow, both for fresh material and for fixed smears. It does not matter in the least that in our own laboratory there are two opposing views concerning the interpretation of such specimens, we believing that the clasmatocytes and monocytes represent two different functional strains of cells, while Dr. and Mrs. Lewis believe that they are one strain. Indeed such differences in interpretations are as stimulating and valuable to our students as they are to ourselves. When you can bring out such points of difference of real significance to the study of what cells actually do, then the student knows that he is studying a growing zone of medicine. Such differences are only of value to a student when both points of view are presented to him with entire fairness. For our own work we regard that the separation of these two strains of the cells of the connective tissue is of value because we were also working on tuberculosis last spring and obtained evidence which convinced us that it is specifically the monocyte which becomes the epitheloid cell in tuberculosis. This concept seems to us to open up an entirely new range of experimentation in connection with tuberculosis, in which it will be possible to survey the whole subject of the relationship of clasmatocytes, monocytes and lymphocytes in this disease.

The next subject which I wish to discuss is the teaching of the pancreas. Here we would follow the work of Dr. Bensley. We would give our student sections of the pancreas, selected in different functional states, as Dr. Bensley has taught us, to show the maximum of the basophilic substance and the maximum of zymogen granules. For the class work we would give a bit of the fresh pancreas of a rabbit, and sometimes a second preparation after an injection of pilocarpin to show an emptying of the zymogen granules. We would then give the pancreas of the guinea pig with the vital injection of neutral red for the islets, have the experiment of the counting of a small bit of the organ, and hunt for the individual islet cells either in the septa or from more completely teased bits of the tissue. On the next day we would give the vital injection of the Janus green for the mitochondria, the beautiful demonstration of the controacinar cells by the injection of methylene blue and finally the mixed injection of pyronin and Janus green. Ever since Dr. Bensley published his studies on the pancreas we have tried for a satisfactory demonstration of the blind ducts to the islets, and this year we got such a clear demonstration of the point that no student could miss it. By chance we made an injection of the pyronin of double strength combined with Janus green and let the preparation stand a half hour; then every islet was clearly stained in the Janus green and the plexus of the ducts stood out beautifully distinct in the pale red of the pyronin. I think that no student could look at the demonstration of the living centroacinar cells with their processes reaching far out between the acinar cells without realizing how inadequate ordinary sections are to demonstrate them. After a student has made such a study of living pancreatic cells, sections may seem a little dull to him, but they will serve to remind him of what the pancreas really looks like and we are confident that the student would still have some vivid memories of the real organ in his mind to aid him in physiology and pathology.

The fourth illustration which I wish to give is from the reproductive system. Our material for this subject would depend on the work of Ancel and Bouin, Stockart, Evans and Long, and Corner. It happens that the laboratory in Baltimore is near a slaughter house and so an abundant supply of material from the pig is available. We would give each student or each two students an entire uterus and ovaries from a pig. Then, following the work of Corner, we would have each student study the ovary with reference to its condition in the oestrous cycle and whenever an ovary

was found with a recently ruptured follicle, the tubes or the uterus would be washed out by Corner's technique and the ova recovered. Frozen sections would be made from a corpus luteum selected to show the greatest amount of fat and the sections stained with Sudan III. On the next day each student would receive a pregnant uterus and study the foetal membranes and placentation. In our course this day's work was developed by Dr. Wislocki, who gave a demonstration of comparative placentation from his own work. Each person teaching in a course should thus enrich it with the material of his own research. After such a study in the laboratory the student would not find it hard to study sections of ovaries; all sections of the ovary and of the uterus of whatever animal used should now be given out with a record of what phase in the cycle they represent. It seems to me anatomists have contributed more in the past few years to the analysis of the reproductive cycle than all clinical experience put together.

I trust that you will pardon the detail with which I have given these illustrations; they show how the material for a course in histology can be built up from research and they represent material that is entirely feasible from the standpoint of experience. It is all readily obtained and not too difficult to analyze. I have intended these examples to show the difference between an elementary and an advanced course in histology. Such plans for material I am convinced should be frequently changed in any course, if it is to have life and vigor. Sometimes a wholly new arrangement of the sequence of a course will give new plans of presentation, or a new technique will develop in research that will give a whole new range of material. Material should be frequently dropped from a course and new specimens brought in. In a word, it is of more importance that the material of a course should be thoroughly interesting to the teacher himself than that he should try to cover the entire ground of his subject each year.

I am quite aware that besides the general criticism of this concept of the type of course in histology, there will be the direct criticism of the pathologist that in such a course the student does not get such a specific knowledge of the characteristics of normal human material as to give him a nice discrimination of early pathological changes. In this connection it is my judgment that courses should not be too closely dovetailed because in the medical school the student gains by having similar subjects from different viewpoints. I think that the histologist can better afford to use sections from different animals so selected as to demonstrate function in the clearest way possible or else selected to show variations in structures of the different experimental animals, such as the marked variation of bronchial musculature, that are at the present time significant in medical research and leave to the pathologist the human material, over which he has such a vastly greater control and concerning which he is almost certainly more competent to judge.

It has come to my attention in late years that the directors of physical training in all secondary schools, even those for girls, receive a higher salary than any of the very most successful of those teachers who are concerned only with the intellectual side of education. I have finally found some comfort in this unwelcome fact in the thought that at least no teacher of physical exercise commits the cardinal sin of thinking that he can develop the muscles of his students for them by exercising his own muscles. I want to stress the idea that the value of a laboratory to the student is that it is a place where he can work. The student could get facts more quickly out of a book; a text-book that tends to take a student away from his microscope is a poor aid to a course in histology. Moreover, the teacher does not need to point out to the student the things in his section which the student can find for himself. For example, when the student is studying blood-vessels the teacher may show him a corrosion of the organ and call his attention to the number of orders of vessels in a given layer; he should show him how to dissect with the focusing screw of the microscope the coats of vessels in some such membrane as the pia mater, but he does not need to find for him the vessels in sections. Most of all the teacher does not need to concern himself with aiding the student to memorize; for that the student can be entirely responsible himself. If I were to give a young teacher advice concerning the directing of a laboratory course, it would be this; see that the material is carefully chosen and excellent in quality before it is given out, remember most of all that the period of laboratory belongs to the student for his work; when you do demonstrate to a student let it be to show him that there is much more in his material than he has been seeing, that research involves careful observations and records as well as interpretation, and when you leave him be sure that he has a fresh impetus for his own work.

There is a double problem in all medical teaching; medical schools are founded to train physicians, who shall practice medicine, and research workers, who shall advance medical science; that these two groups are not entirely distinct all medical history attests. I do not minimize the importance of training practitioners of medicine, indeed in all schools they make the larger group of the students, but it is entirely clear that unless teachers of medicine attract and train those who are to be teachers in the next generation, the profession will die at its source. But, while any given school will train only a small percentage

of its students to become professional investigators, nevertheless all teaching in the sciences can and should depend on the methods of research. All the class can be taught in the spirit of research, which means that it is more important for the student to be able to find out something for himself than to memorize what someone else has said. Does any one doubt that this principle applies to diagnosis? Certainly in the practice of medicine every case is a problem; and therefore a plan of instruction for the practice of medicine that allows great range for the idea of solving problems must be the ideal one. The knowledge of what has been done must play a large rôle in the training for any profession; nevertheless, the ability to solve a problem is so much more difficult to acquire than the ability to memorize facts that education should stress the one even at the expense of the other. Indeed the facts which one remembers from one's own experience or from the experience of one's profession are merely the tools with which to work.

The ability to catch the imagination of young people and direct their interests toward research is quite a special gift in a teacher and each school must have some members of its staff who enjoy this type of teaching if that school is to contribute toward the training of research workers. In the days when I first started the study of medicine, the attitude toward the research student was entirely different from what it is now. Then, from the student's standpoint, it was unpopular to do research, and the one who undertook it had little sympathy either from his home or from his fellow-students. At present most of the outside influences to be summed up under public opinion favor research. Nevertheless, I believe that the best method of getting students interested in research is now the same as it was then, namely, through an interest in ideas presented by a teacher who has the power to catch the imagination of the gifted student. It may be that the student will apply for research, it may be that the teacher will suggest a problem to a student, but it is essential that student and teacher shall be in sympathetic understanding. The extent to which the student's mind will be turned toward research is in direct relation to the quality of intellectual leadership on the part of the teacher; this quality is brought out both in lectures and in informal discussions with students. In connection with lectures it is my fixed opinion that they should be few in number and that they should not aim at giving any comprehensive body of approved facts such as are rightly set down in text-books, but they should deal with the ideas in which the subject is growing. A static subject needs no lectures.

In all the discussions concerning a comparative analysis of the differences between European educational systems and ours, the point is brought out that in Europe the student has much greater freedom of choice and sequence of courses than we permit, and, correlated with this freedom of choice, is his vastly greater practice of wandering from one university to another. In Europe the best students wander freely. I can see great advantages in freedom of choice for the student; in essence it enables him to choose his teacher. I should like to see a student able to try medicine, then law or engineering on his own initiative, to find out what he really likes best without any feeling of failure in his mind; nevertheless, I believe that there is a point of far greater significance to education than the discussion of freedom of choice of a course and this point is freedom within the course. I happen to know that one of the most famous of the European courses in histology was conducted in the following manner: the teacher sat in the center of the room, with a slide and a microscope, and his two hundred students sat around him, each with a microscope and a section from the same block and each student followed his section under the microscope while the teacher described it. That is drilling, not leading; that it gives thoroughness of information and respect for authority, I do not doubt, that it satisfies the average student, I grant. I must say, also, that it gives an illustration, that there is no one method of teaching, for it is true that the nation in which large classes were taught in this way has produced more investigators than we have done. But the system was saved by the fact that there were other factors in the community as a whole that favored research, so that there were those whose interest survived the system, and, when they undertook research, they were then given the utmost freedom. Nevertheless, I believe that the concept of freedom for intellectual life on the part of the student within the course is a far greater type of teaching. Rules of how to teach can not be given, but the negative side can be expressed. I do not believe that a teacher gets the best out of students when he presents scientific facts to them on the basis of his own authority rather than as the shifting inferences from evidence. Why we think so, not just what we think, is the material of instruction. There is no one method of teaching, but its essence is to lead the student to want to use his own mind. It is a much more subtile thing than showing a student how to do a physical exercise, but I believe that an entire class should be taught in such a way that from the teaching itself the abler ones will of their own initiative desire to do research. Teaching in a professional school I should regard as advanced, not elementary instruction. Freedom within the course involves much; it means eliminating quizzes, eliminating all

following of the student day by day to see if he has done this task or that; the student's work will be his own business and will be done at his own initiative. His note books and his records will be for his own instruction. In a word, such freedom will mean treating the students of a class as you treat research students, discussing ideas with them, suggesting plans of work, putting your greater experience to his advantage. In lieu of quizzes, a group of students could often be advised to organize and to run small clubs of their own in which they could look up and present work exactly as they will do later in professional life. If such a group had one or more members who were doing research themselves and would present it to their fellows, I should regard it as a great stimulus. The proper teaching of a course would give many suggestions of topics concerning which the students would themselves desire further study.

Freedom within the course, then, I should offer as a major subject for discussion in education. It is clear, I think, that, in a laboratory course, we can not mean, by freedom within the course, the utmost liberty on the part of each student to select his own material. Here he should be glad in the first place to profit by the teacher's greater experience. Moreover, a research student can have a degree of freedom with regard to material that is not feasible for a large class. There are two drawbacks that prevent such complete freedom for a class. The first of these is the work and the expense of preparing the material for a laboratory course. This is not true for the sections for a course in histology; they are usually cut in the summer months before the work is taken and could be given out to the student at any time. But it is true of the fresh, experimental material, which must often be used at exact times after it has been prepared and no laboratory can afford enough of the teacher's time or enough animals and materials to allow the student to follow his own sequence. Thus material in a laboratory forces the students to work on the same general topic at the same time. Secondly, no teacher is really equipped to lecture on a subject without fresh study and fresh reading each year. Everyone knows that we do not attempt to remember all the details of our subject, we know that one of the advantages for us in teaching is that each year as we take up each subject in sequence we find out what has been done in it, we study and think about it and above all try to formulate whether we can see some new opening to clarify it. Thus, from the standpoint of material and from the standpoint of the limitations of the teacher himself, it would be difficult to offer to large classes of students the liberty to choose the sequence of material to be studied within a course, but, recognizing this limitation, how much more important it is that he should have the utmost intellectual freedom in studying the material. It comes to this, then, that the liberty that is most essential in education is intellectual liberty, freedom of thought. Everything else is secondary to this. Dogmatism should be ruled out of teaching; the student should be made to see that a book is merely a record of what some one else has observed and thought about the same material that is offered to him for study and that the material itself is the safer guide. You will assuredly have some difficulties if you discuss ideas of intellectual freedom with students; if you tell them that it is more important to think than to memorize, for they will have some very specific experiences of examinations which were pure memory tests and for the most part they will discount your sincerity.

One of Dr. Mall's most favorite sayings was that an examination is more of a demonstration of a teacher's type of mind than a test of a student's ability. To quote Miall's again, "An examination paper should not be such that it could be handsomely answered by a candidate who had free use of a textbook. Papers of that sort are very common and encourage the verbal memory too much." What shall teachers do to counteract the reputation of their profession for dogmatism but to discuss liberty in education in the freest possible way, especially with their own students.

I wish now to discuss why I think that histology has become such an important subject in medical research. It is because histology has become the experimental study of the living cell. When we seek to trace this transformation in our subject we shall find that many influences have brought it about, but in evaluating the chief factors, I shall give first place to the introduction of the method of tissue culture by Professor Ross G. Harrison. As we all know, Dr. Harrison developed this technique for one purpose, namely, to analyze the nature of those remarkable processes that grow out from the cell body of the neurone, but the method has ushered in a whole new period in medical research. We all know of the application of this technique to mammalian tissues grown in plasma by Dr. Montrose Burrows working in Dr. Harrison's laboratory, of the further development of the technique and of the results attained by Dr. Carrel at the Rockefeller Institute, of the simpler technique of Dr. and Mrs. Warren Lewis, by which tissues can be grown in a fluid medium, which may not give as high a percentage of growth as the plasma method but which gives extraordinarily clear pictures of cells. There are many other factors in the transformation of this subject, the experimental embryology of Born, the study of living eggs by the biologists, the early

study of living lymphatics in the tadpoles' tail by Dr. and Mrs. Clark, the study of living gland cells by Dr. Bensley, the introduction of the so-called vital staining by Goldmann, which has assisted in the experimental analysis of phagocytosis, the technique of supravital staining of cells and many other procedures. All these investigations have made a new subject out of anatomy, but they have done something much more important, they have broken down the artificial barriers between the different medical sciences. These things are not just the technique of anatomy but rather of all those who use the microscope. In the premedical sciences we had separated ourselves off into many groups for the purpose of teaching, on account of the development of different elaborate techniques; but we are now having a new realignment in the medical sciences. This realignment is actually taking place through a natural evolution in research; the type of problems which are being analyzed at the present time are obviously much more complex than those of a few years back.

Thus medical science itself is undergoing some such transformation as I have tried to describe having taken place within the one subject of histology. The great descriptive phase of medicine, when physicians were concerned with the classification of diseases, is drawing to a close; to-day we have as examples of major problems the nature of immunity, the analysis of how the cancer cell differs from the normal cell and the fundamental analysis of nutrition. It is directly in relation to such general phases of medical research, problems for research of interest to the whole body of the medical profession, that it seems to me that histology is playing a rôle. It is because, to use a specific example, one great phase of the subject of immunity is now specifically the experimental physiology of the different types of the cells of the connective tissues that the medical profession as a whole can with profit come back to the most ancient of its sciences, anatomy, for one of the newest of its scientific techniques. In actual research at the present time we have now in the premedical sciences not six separate scientific branches, but three great groups of workers, more or less distinct, on the basis of technique, those who use predominately the microscope, those who use the test tube and those who use the kinograph as a symbol of instruments for measuring mass action. Does not such a statement show how the barriers have already been broken down between the premedical sciences?

In the great period of the specialization and isolation of the sciences through which we have just passed we have developed a great elaboration of nomenclature. So complex has scientific nomenclature become that names rather than the actual complexity of the subject serves to wall off a given science from all other scientists. The complexity of nomenclature will certainly be one of the factors of greatest difficulty in the organization of the new project of biological abstracts. We must, it is true, have some specific names to be exact, but in the interests of the whole body of scientific workers in biology could we not insist that the titles of all papers should be so simple that any educated person might be able to tell what the paper is about, and the text so clear that any scientist could understand it? This would not involve giving up the exact terminology of science, but it would involve giving simple synonyms; for example, no anatomist can keep in mind all the names of zoology, but he is immensely aided just to know that carcinus is a crab. Let us get together on ideas rather than separate on nomenclature.

If you compare the titles of scientific articles in the journals of to-day with those of twenty years ago you will find that we are beginning to work in groups, often quite large groups. The nature of research is changing in such a way as to make this necessary. If you will allow me to illustrate from my own experience, Dr. Cunningham, Dr. Doan and I have been making some studies on the rhythms of the white blood cells. We have taken an individual and made studies of his blood every fifteen minutes all day long. Every fifteen minutes we have made a total count of the red cells, a total count of the white cells, a differential count of the white cells by the vital technique, a count of the platelets and of the reticulated red cells as well as a differential count of the white cells from fixed films. We have found that there is an hourly rhythm of the white blood-cells, that every individual covers the entire range of the normal number of cells per cubic millimeter, roughly, 5,000 to 10,000 cells each day, that the white cells are more numerous in the afternoon than in the morning whether the individual takes lunch or not, that the neutrophilic leucocytes have an hourly rhythm and die out in great showers in the circulation, that the lymphocytes have a shorter rhythm and do not die in the circulating blood to any appreciable extent and that the variations of the number of the different types of the cells are much greater than has been suspected and yet the rhythms are remarkably constant. It is quite obvious that such an experiment is entirely impossible without joint research; without a group of workers it would be left undone, in fact, we had considerable help beyond our group of three.

This is an example of joint research in which all use the same technique, but we are also having joint research between different groups. Perhaps one of the most significant examples in modern medicine of a joint attack on a major problem has been given us

by the National Association for Tuberculosis in this country. A committee of that association has invited a group of workers in the different fundamental sciences to cooperate with them in an extensive study of tuberculosis; and this committee is showing the most far-sighted spirit of cooperation in putting the work of each division to the advantage of all the rest. It might be a matter of interest to us as anatomists that three of those who have been chosen for this work are members of this association.

Another most interesting example of such a correlation is to be found emanating from the chemists who have been studying rickets. The chemists have obtained cooperation of both histologists and clinicians with the greatest possible advantage to the solution of their problem. So many other examples could be found in the various institutes for research as to demonstrate quite conclusively that one of the most striking features in the research of our time is the development of cooperation.

The nature of joint research introduces a new factor to the problem of the training of students to become investigators. In my judgment the most successful method of training students in research in the past has been to start a student with a problem in which he could work by himself, could make his own specimens and analyze them; in joint research each one has to be a cog in the wheel during the time of the experiment; to put a student into such a group, to let him work with you on the general problem is really the time-honored method of the apprenticeship. That an apprenticeship has worked well in the arts, numerous examples of the student who has surpassed his master demonstrate, but I can not but feel that, though the method must often be accepted on account of the necessities of our problems, it calls for extra safeguards lest the student become a mere technical assistant and get no start in independent work himself.

There may be, of course there will be, personal difficulties in joint research, but certainly they will be no worse than our old scientific controversies. I have had some experience with scientific controversy, and in my judgment it is a poor technique. The flaw in it is this, that in controversy one seeks to convince one's opponent by argument and evidence that one's own theory is right and his theory is wrong. Now this is the exact thing that is not worth doing; because as long as a subject is growing, or in order that it may grow, it is of the utmost importance that people should be working on different theories. The worst effect of controversy is on the individual who takes part in it, because it tends to force him into contracting a marriage with his own theories; assuredly he should be in love with his theories, but he should never promise to remain so. Controversy is entirely different in spirit from the stimulating discussions between different workers that are the real basis for the formation of scientific societies. Herein lies a subtle difference in quality which may perhaps be expressed as follows: discussion stimulates while controversy depresses.

I offer then two topics for your consideration, liberty of thought in education and cooperation in research.

FLORENCE R. SABIN
THE JOHNS HOPKINS MEDICAL SCHOOL

RESTORATION OF THE OLDEST KNOWN FOREST

RESEARCH work carried on by the geological staff of the State Museum at Albany under the direction of Dr. John M. Clarke has resulted in the opening in that museum, on February 12, of a unique and remarkably realistic exhibit of a restoration of a forest of late Middle Devonian Age. Occupying a central alcove about 35 feet long, 30 feet high and 25 feet in depth, this exhibit has been rendered possible by the great wealth of fossil trees that, during the past four years, have been collected in the vicinity of the little village of Gilboa, N. Y. In 1869 a very heavy autumn freshet in the upper valley of the Schoharie creek in the Catskill mountains exposed a number of fossil tree stumps in the bed-rock close to the creek. The fossils collected at that time by Professor James Hall were submitted for investigation to Sir William Dawson, then principal of McGill University and the leading paleobotanist of his day. When one considers the character of the material he was called upon to examine, his observations were remarkably accurate, although he was unable to establish in detail the true nature of these trees.

Since 1920, excavations by the New York Board of Water Supply for the construction of a dam on Schoharie creek near Gilboa have exposed three horizons of erect fossil tree stumps within the sandstones and shales of that locality. At no other place in the world has there been discovered in rocks as old as these any approach to a similar abundance of fossil trees in such a remarkably complete state of preservation. From these three horizons, each of which is separated from the one next above by 60 feet of barren sandstones, there have been collected sixty or more stumps and many portions of the trunks, branches and roots of these trees, as well as their foliage, seed-bearing capsules and sporangia-bearing organs. At each horizon the more or less bulbous stumps, up to three feet in diameter, rise from black shales representing the rich soil in which the trees grew; in fact,

from some of the stumps, branching roots and rootlets have been found extending laterally within the shale for more than six feet. From this abundance of plant remains and the mode of their appearance in the enclosing rocks, it has been possible to vividly portray not only the trees as they were when living but also the character of the landscape they clothed.

In the leaflet prepared for distribution on the occasion of the opening of this exhibit, Dr. Clarke writes:

> They were majestic trees, simple in structure, resembling closely the tree ferns of the present tropical jungles but singularly enough an advance in structure over these tree ferns by virtue of their seed-bearing devices. The Gilboa forests grew along the low shores of the western Catskill mountain region, facing the interior sea which at that period covered all of Central and Western New York. They grew in marshes or jungles along these lowlands which were easily covered by the rise of the tides. Their swollen roots were anchored in soft black muds, and the streams running down from the land found a meandering course among them out to the sea. The atmosphere of these coastal marshes must have been dank and heavy, and the rays of the sun sifted down among them only with softened light. They were places where the vegetation grew rank. Along with these Gilboa trees, which have now been given the name of Eospermatopteris, grew a few other plants, some simpler ferns and a strange lycopodium tree, Protolepidodendron, or the Naples Tree, as it has come to be commonly known.

The foreground of the exhibit illustrates the manner in which the fossil trees occurred at Gilboa, and shows fifteen of the tree stumps distributed along the three successive horizons of dark shales with intervening sandstone beds. In the background, full-size restoration of the trees in various stages of their growth blend with a beautiful panorama of the forest as it must have appeared in Devonian times. Water trickling down the cliffs in the foreground gathers in a pool which gives continuity to the swampy conditions depicted in the vista of the background. As a geologist views this masterpiece exhibit, his thoughts are those of satisfaction that so vivid a picture of a forest, earlier than any hitherto discovered, has been won from the record of the rocks, mingled with the hope that further discoveries may be made which will dispel the mists of the background and reveal the vegetation of even earlier periods of the earth's history.

In vividly portraying the life and conditions which existed during the Devonian period, Dr. Clarke has carried the torch forward far in advance of its position in Sir William Dawson's day, yet, "as a memorial of the admirable service rendered by Sir William Dawson to the science of paleobotany, and as a record of his personal association with the original discovery and study of these trees," Dr. Clarke has generously and appropriately chosen "to dedicate this exhibit as a testimonial to him."

J. AUSTEN BANCROFT
McGILL UNIVERSITY

JOHN VAN DENBURGH

1872–1924

DR. JOHN VAN DENBURGH, distinguished herpetologist and curator of herpetology in the California Academy of Sciences, San Francisco, died in Honolulu, October 24, 1924.

He was born in San Francisco August 23, 1872. His father, Dr. Daniel Van Denburgh, was of Holland Dutch stock that came to America at an early date, settling in New York state. His mother was Elizabeth Douglas Turrill, eldest daughter of Judge Joel Turrill, who was for several years United States consul at Honolulu. Elizabeth Douglas Turrill Van Denburgh was of English descent, her ancestry going back through ten generations to John Mather, of Lowton, Winwich Parish, Lancashire. The first American ancestor was the Reverend Richard Mather, who came to America in 1635.

Soon after their marriage at Syracuse, New York, in 1863, Dr. Daniel Van Denburgh and his wife came to San Francisco, where he was, up to the time of his death in 1911, a prominent dentist.

Early in John's childhood the family acquired a country home at Los Gatos, Santa Clara County, California, some 50 miles from San Francisco. It was there that the subject of this sketch spent his boyhood years and grew to manhood; and it was doubtless there in the beautiful environment of forest, hillside chaparral, mountains and wide expanse of valley that his interest in natural history developed rapidly and enduringly. Like many another boy who in manhood attained eminence in some field of zoological or botanical science, John Van Denburgh's first interest was in birds and their nests and eggs, his first published paper, which appeared in *The Oologist* in 1888 when he was scarcely sixteen years old, being entitled "Two large sets of quail eggs." His interest in ornithology and oology continued more or less intermittently all his life. While a student in the University of the Pacific in 1890 he, with three other congenial associates, organized the Cooper Club devoted to the study of birds, their nests and eggs. When Stanford University opened in 1891 most of this group (including Van Denburgh) entered that institution and the Cooper Club ceased to exist, temporarily at least, until June 22, 1893, when it was reorganized by W. H. Osgood (now Dr. Osgood, curator of zoology in the Field Museum, Chicago), and a few other boys interested in birds.

During his college days at Stanford University, Van Denburgh's interest in natural history continued unabated. He was an organizer and charter member of the Stanford Zoology Club, which has continued to this day as one of the most active and potent factors in promoting and maintaining high ideals and a real scientific natural history spirit in that institution.

Van Denburgh graduated at Stanford in 1894, when he received the degree of A.B. In 1895 he received the degree of A.M., and in 1897 that of Ph.D.

During his college course he became interested, through his major professor, Dr. Charles H. Gilbert, in herpetology, and his most notable contributions to science have been in that field.

In the winter of 1896–97, he began a series of experiments with the saliva of the Gila Monster. He wished to determine (1) whether the bite is poisonous; (2) if poison is present, in which jaw is the poison located and what are the physiological effects; and (3) what are the causes of such diversity of opinion regarding the poison of this reptile. These investigations were carried out with remarkable skill and thoroughness and with infinite patience. A total of more than thirty experiments were performed, each with adequate controls. Pigeons and small mammals were used, the saliva or venom was injected subcutaneously, and the progressive effects very carefully recorded.

It was found that the saliva from the upper jaw had no toxic effect in any of the tests; it was found to be harmless at all times. On the other hand, the saliva from the lower jaw showed decided toxic effect in every case, the pigeon or other animal in which the saliva was injected usually dying in a few minutes. He thus demonstrated that the poison is present in the saliva of the lower jaw only and that it is capable of killing a pigeon in seven minutes and a rabbit in less than two minutes. From which it was concluded that a venom which would do this might, under favorable circumstances, easily prove fatal to man—"a belief which is rendered far from improbable by the extraordinary virulence of the poison and the lizard's habit of holding like a bulldog to whatever it bites."

That the bite so rarely produces serious results Dr. Van Denburgh showed to be due to the great difficulty of the fangs of the lower jaw causing a wound and of carrying the venom into the wound when caused.

The results of these investigations were published in the Transactions of the American Philosophical Society for 1897 (pp. 199–220), and in *American Journal of Physiology*, Vol. 4, No. 5 (Sept. 1, 1900), and are the most important contributions to the literature of this subject.

Dr. Van Denburgh first became connected with the California Academy of Sciences June 2, 1894, when he was engaged to look after the collections of fishes, reptiles and amphibians belonging to the academy.

On May 6, 1895, he became curator of the department of herpetology, which position he occupied (except during the period when he was in medical school in Philadelphia and Johns Hopkins), until his death.

He received his M.D. degree at Johns Hopkins in 1902 and returned to San Francisco where he entered upon the practice of medicine. He also again became curator of herpetology in the California Academy of Sciences, and it was to that department that he devoted most of his time and strength. He was always resourceful and active in looking after the interests of his department with the result that, when the disastrous fire of 1906 occurred, the collections in herpetology (practically all of which were lost in that fire) had become among the largest and most valuable in the academy. Immediately following the fire, Dr. Van Denburgh, with remarkable energy, at once began rebuilding the department of herpetology with the result that the collections in that department now number more than 56,000 specimens, a number believed to be greater than in any other institution in America, with possibly two exceptions. It was Dr. Van Denburgh's ambition to make the California Academy of Sciences the most important center in the world for research studies in herpetology, and there is good reason for believing this ambition would have been realized had he lived.

Although Dr. Van Denburgh's interest in ornithology never ceased he was not always able to give to it the time and attention he would like to have given. Nevertheless, he found time to contribute a number of short papers to various ornithological periodicals and to assemble an unusually well-selected oological collection, numbering 4,667 eggs or 1,311 sets, many of which are of great interest and value. This collection is now the property of the California Academy of Sciences, in accordance with Dr. Van Denburgh's wishes.

It is, however, through his studies of reptiles and amphibians and his contributions to herpetological literature that Dr. Van Denburgh is best known to naturalists. He was interested in the life histories of snakes, lizards, turtles and amphibians, and in the problems of geographic distribution in their relation to the problems of taxonomy.

His most notable contributions to herpetological literature, besides those relating to the venom of the Gila Monster, already referred to, were chiefly taxonomic and faunistic in character. His published papers total nearly one hundred. Perhaps the most pretentious and most useful is the monograph on "The

Reptiles of Western North America," published by the California Academy of Sciences in 1922. This is a monumental work in two volumes, 1,028 pages and 128 excellent halftone plates, indispensable in the study of western American herpetology.

Dr. Van Denburgh's wide experience as a field naturalist, his knowledge of comparative anatomy and physiology, his expertness in laboratory technique, together with infinite patience and a genius for the interpretation of taxonomic relationships, enabled him to do research work of the highest order and to place him among the most able herpetologists of his time. His death while yet in his prime is an irreparable loss to the California Academy of Sciences and to herpetological science, as well as to his friends and coworkers.

BARTON WARREN EVERMANN
CALIFORNIA ACADEMY OF SCIENCES
SAN FRANCISCO, CALIFORNIA

SCIENTIFIC EVENTS

MOSELEYUM AND THE NAMES OF ELEMENTS[1]

IN the issue of SCIENCE for February 20, Professor Richard Hamer, of the University of Pittsburgh, Pa., enters a plea for naming the missing element of atomic number 43 before it is discovered; in view of the work of Bosanquet and Keeley (*Phil. Mag.*, 1924 (6), 145–147) and of others, he thinks that the discovery can not be long delayed, and also that by taking time by the forelock in this manner, subsequent controversy, like that which followed the discovery of hafnium, will not arise. Professor Hamer appeals to the scientific world to name the element "Moseleyum," in honor of the young British physicist who fell in Gallipoli, and to give it the symbol "Ms." In our view it would be a fitting tribute to the brilliant work of Moseley to perpetuate his name in some such way. Hitherto, no chemical element has been named after an individual (we exclude mercury, tantalum, thorium and titanium for an obvious reason), and opinion may be divided on the advisability of making the innovation. It is, however, a mistake to be bound by precedent in such a matter, and the only objection we can foresee to the adoption of Professor Hamer's suggestion is that the word is not particularly euphonious, and is rather suggestive of certain sepulchral monuments; but it might be argued that even this suggestiveness is not inappropriate, inasmuch as mausoleums are erected, as a rule, to the memory of the illustrious dead.

Referring to our note (*Nature*, April 11, p. 545) on Professor R. Hamer's suggestion to name the undiscovered element of atomic number 43 "moseleyum," after H. G. J. Moseley, Professor Irvine Masson writes that such action would not, as stated, be an innovation, as "one element *is* named after an individual: namely, Gadolinium, a rare-earth element, called after Gadolin." The historical facts appear to be as follows: The mineral gadolinite, discovered in 1788, was named after the Finnish chemist Johann Gadolin, who in 1794 discovered a new earth—yttria—in it. About a century later, Marignac showed that yttria (which he had obtained from samarskite) contained a new element, and when Lecoq de Boisbaudran announced to the Paris Academy of Sciences that Marignac had chosen for it the name "gadolinium," he gave no reason for the selection (*Comptes rendus*, 1886, p. 902). The case of the element samarium is somewhat similar. The complex parent mineral samarskite was, apparently, named after a Russian mine officer, Samarski. When the existence of the element was proved, Lecoq de Boisbaudran told the academy that the honor of its discovery really belonged to several investigators, and he proposed the name "samarium" because the word was "derivé de la racine qui a déjà servi à former le mot 'samarskite'" (*Comptes rendus*, 1879, p. 214). Whether the words "gadolinium" and "samarium" were derived directly, or indirectly, from the names of men or minerals appears of little moment; both perpetuate the names of individuals, and therefore, in this sense, the appellation "moseleyum" would have two precedents.

[1] From *Nature*.

THE AMERICAN ASSOCIATION OF MUSEUMS

THE American Association of Museums is holding its twentieth annual meeting at St. Louis. Sessions begin on Monday, May 18, and continue until Thursday noon. The Sunday preceding the meeting is to be the occasion of a sail down the Mississippi River and of other hospitalities tendered by the local committee.

The purpose of the association in choosing St. Louis for its meeting place is to assist a local group in efforts to establish a science museum in that city. St. Louis already has an art museum, a history museum and the best known school museum in the world. The last institution, the Educational Museum of the St. Louis public schools, is organized under the department of education and is devoted exclusively to school service.

The program of the St. Louis meeting follows:

SUNDAY, MAY 17

10:00 A. M. Registration at the Chase Hotel.

11:00 A. M. Visit to the Zoological Garden. Busses will take members and guests from the hotel.

12:00 M. Luncheon on Garden Lawn. Guests of the officers and members of the Zoological Society.

1:00 P. M. Trip down the Mississippi River. Guests of the local committee. Busses will take the party from the Zoological Garden to the harbor boat.

MONDAY, MAY 18

10:00 A. M. General session at the Chase Hotel.

Presiding: President Chauncey J. Hamlin.

Address of Welcome. John C. Tobin, president of the Board of Education.

Culture museums and the use of cultural material: Laura M. Bragg, director of the Charleston Museum.

2:00 P. M. General session at the Chase Hotel.

Training of museum workers. Report of the committee on courses in museology and discussion led by Dr. S. A. Barrett, director of the Milwaukee Public Museum.

Museum finance and accounting. Report of the committee on finance and accounting and discussion led by Paul Marshall Rea, director of the Cleveland Museum of Natural History.

Museum ethics. Report of the committee on ethics and discussion led by Harold L. Madison, curator of education, of the Cleveland Museum of Natural History.

8:00 P. M. General session at the Chase Hotel.

The industrial museum. Illustrated lecture by Professor Charles R. Richards, director of the American Association of Museums.

8:45 P. M. Round tables. The three committees which reported in the afternoon hold simultaneous session to which all are welcome.

TUESDAY, MAY 19

10:00 A. M. General session at the Educational Museum of the St. Louis Public Schools.

Cooperation of educational resources of a community: Frederic Allen Whiting, director of the Cleveland Museum of Art.

Discussion. Presiding: Mr. Whiting.

11:30 A. M. Inspection of the museum.

13:30 P. M. Busses leave for the Missouri Botanical Garden.

1:30 P. M. Luncheon. Guests of the directors and the board of managers of the Missouri Botanical Garden. Inspection of the garden after luncheon.

7:00 P. M. Banquet at the Chase Hotel.

WEDNESDAY, MAY 20

10:00 A. M. General session at the City Art Museum.

Museum lighting. Symposium. Contributions by: Harry Ivan Day, New York, N. Y., S. Hurst Seager, F.R.I.B.A., past vice-president, New Zealand Institute of Architects, Benjamin Gilman, Boston, Mass.

Discussion and demonstration of principles of lighting: L. C. Kent, engineering department, National Lamp Works, Cleveland.

11.30 A. M. Inspection of the museum.

2:00 P. M. Trip to Cahokia mounds in Illinois.

Story of the mounds: Dr. Henry M. Whelpley, St. Louis.

6:00 P. M. Dinner meeting of the new council at the Chase Hotel.

8:00 P. M. General session at the Chase Hotel.

A program for Historical Society Museums: Dr. Arthur C. Parker, director of the Rochester Public Museum.

8:45 P. M. Round tables.

Art museum problems. Presiding: Robert B. Harshe, director of the Chicago Art Institute.

History museum problems. Presiding: W. H. Cathcart, director of the Historical Society of Cleveland and the Western Reserve.

Science museum problems. Presiding: Dr. Arthur C. Parker, director of the Rochester Public Museum.

THURSDAY, MAY 21

10:00 A. M. General session at the Jefferson Memorial.

Museum and School: Dr. Carl G. Rathmann, director of the Educational Museum of the St. Louis Public Schools.

Discussion. Presiding: Dr. Rathmann.

11:30 A. M. Inspection of the memorial: Home of the Missouri Historical Society.

After luncheon the local committee will take the members and guests by automobile through the parks and surroundings of St. Louis.

PHARMACEUTICAL RESEARCH

AT the 1924 meeting of the National Conference on Pharmaceutical Research the Executive Committee of that organization was directed to take a census of pharmaceutical research. Accordingly a questionnaire was prepared for distribution and was sent to some 270 persons interested in pharmaceutical research. These question sheets brought returns which when tabulated recorded the names of 239 research workers who may be classified as follows:

Hospital pharmacists	3
Retail pharmacists	10
Pharmacists engaged in medical school and chemical school work	16
Governmental scientists doing pharmaceutical research	19
Non-pharmaceutical teachers and students doing pharmaceutical research (chiefly on the synthesis of medicinal chemicals)	28
Manufacturing pharmacists	72
Pharmaceutical teachers and their students	91
	239

The complete list will be published in a forthcoming issue of the *Journal of the American Pharmaceutical Association* and a copy of it will be sent to any person writing H. V. Arny, chairman of the National Conference on Pharmaceutical Research, 115 West 68th St., New York City.

NATIONAL RESEARCH FELLOWSHIPS IN MEDICINE

AT a meeting of the medical fellowship board of the National Research Council held in Washington on May 1, twenty-four fellows were reappointed and new appointments were made as follows:

NAME	FIELD	LOCATION
Richard M. Brickner New York	Pathology	W. G. MacCallum Johns Hopkins
Francis D. Gunn Buffalo, N. Y.	Pathology	Benj. Roman and H. U. Williams Univ. of Buffalo
Franklin Hollander Baltimore, Md.	Biochemistry	L. B. Mendel Yale
Homer D. Kesten Los Angeles, Calif.	Pathology	Jas. W. Jobling Columbia
George M. Lawson New Haven, Conn.	Bacteriology	Hans Zinsser Harvard
Clive M. McCay Berkeley, Calif.	Biochemistry	L. B. Mendel Yale
Trevor Owen Baltimore, Md.	Physiology	
I. Theodore Rosen New York City	Physiology	Jos. Erlanger Washington Univ.
Maurice B. Visscher Minneapolis, Minn.	Physiology	A. V. Hill London
Charles Weiss Brooklyn, N. Y.	Bacteriology	F. P. Gay Columbia
Herbert S. Wells Baltimore, Md.	Biochemistry	Carl L. A. Schmidt U. of California
Roy G. Williams New Haven, Conn.	Anatomy	E. R. Clark U. of Georgia

SCIENTIFIC NOTES AND NEWS

IN recognition of the great service rendered to the American Philosophical Society by Professor William Berryman Scott, president from 1918 to 1925, a committee has been appointed with Dr. Henry Fairfield Osborn, as chairman, to arrange for a portrait of Dr. Scott to be hung in the hall of the society.

DR. HENRY M. HURD, emeritus professor of psychiatry at the Johns Hopkins University and first superintendent of the Johns Hopkins Hospital, celebrated his eighty-second birthday on May 3.

WILFRED H. OSGOOD, of the Field Museum, Chicago, was elected president of the American Society of Mammalogists at the annual meeting held recently in the U. S. National Museum in Washington.

DR. B. L. MILLER, head of the department of geology at Lehigh University, was elected president of the Pennsylvania Academy of Science at the annual meeting held in Harrisburg on April 10 and 11.

DR. R. E. LOVING, head of the department of physics in Richmond University, was elected president of the Virginia Academy of Science at the recent annual meeting held at Richmond.

DR. JESSE MORE GREENMAN, of the Missouri Botanical Garden, has been elected president of the Washington University Chapter of Sigma Xi.

AT the annual meeting of the Boston Society of Natural History the following officers were elected: *President,* Thomas Barbour; *Vice-presidents,* Nathaniel T. Kidder, William M. Wheeler and Theodore Lyman; *Councillors for three years,* Henry B. Bigelow, Gorham Brooks, S. Prescott Fay, W. Cameron Forbes, Robert T. Jackson, John L. Saltonstall, John E. Thayer and Charles W. Townsend.

THE Howard Taylor Ricketts prize in pathology has been awarded to Dr. Florence B. Seibert, of the University of Chicago, for her work in "febrile reactions following intravenous injection." The prize is given as a memorial to the discoverer of the germ of spotted typhus fever.

THE gold medal of the Society of Pestology for 1925 has been awarded to Dr. Louis Sambon, for his work on the cockroach as a cancer carrier. Dr. Sambon delivered a lecture on the subject of his researches before the Pasteur Institute in Paris last December.

THE Boston Society of Natural History awards each year the Walker prize, for the best essay submitted on a designated subject in the field of natural history. This year any subject in the field of geology or mineralogy was specified. The prize amounting to $100 was awarded to Edward F. Holden, of the University of Michigan, for his manuscript entitled "The pigment of amethyst."

IT is announced in *Nature* that the gold medal of the Institution of Mining and Metallurgy has been awarded to Dr. Richard Pearce in recognition of his life-long services in the advancement of metallurgical science and practice. Dr. Pearce was for many years engaged in metallurgical work in the United States, and since his return to England has been associated with the tin-smelting industry. The council of the institution has awarded "The Consolidated Gold Field of South Africa premium" of forty guineas to Mr. Thomas Pryor, for his paper on "The underground geology of the Kolar Gold Field," and the "Arthur C. Claudet" and "William Frecheville" students' prizes of ten guineas each to Mr. F. H. Edwards (Birmingham) and Mr. D. W. Bishopp, respectively.

DR. HIDEYO NOGUCHI, of the Rockefeller Institute for Medical Research, has been elected a corresponding member of the Sociedad Medica de Caracas, Venezuela.

DR. RICHARD L. SUTTON, professor of dermatology in the University of Kansas, has been made a fellow of the Royal Society of Edinburgh.

AT a meeting of the administrative board of the American Engineering Council at Philadelphia on May 8, Professor Joseph W. Roe, head of the department of industrial engineering at New York University, was named chairman of a committee to investigate the aviation situation in the United States. The council plans to raise $100,000 for the work of the committee.

NILS A. OLSEN, who has been connected with the U. S. Department of Agriculture since 1919, has been appointed assistant chief of the bureau of agricultural economics. He will have charge of research work in cooperation with state agricultural experiment stations, as provided by the Purnell bill.

DR. PAUL H. DIKE, acting head of the physics department at the University of Vermont, has accepted a position in the research department of the Leeds and Northrup Company of Philadelphia.

JAMES C. CLARK, until recently professor in charge of electrical engineering research at the Iowa State College, has resigned that position to become sales engineer for the Pacific Electric Manufacturing Company, San Francisco, and designer and builder of oil circuit breakers and other high-voltage line equipment.

RECENT appointments to the staff of the New York State Agricultural Experiment Station at Geneva include Dr. R. H. Shriner, a graduate student at the University of Illinois, as associate in research in biochemistry; C. S. Pederson, a graduate student at the University of Wisconsin, as assistant in research in bacteriology to study fermentation problems, and F. G. Gambrel, formerly a research fellow with the Crop Protection Institute, as assistant in research in entomology.

DR. ARTHUR HOLLICK is on leave of absence from the New York Botanical Garden until July 1, during which period he will be in Washington, D. C., continuing his work on the fossil flora of Alaska in connection with the U. S. Geological Survey.

DR. JOHN K. SMALL, of the New York Botanical Garden, is making an extended study of the flora of parts of the Gulf States and Oklahoma and Arkansas. He plans to start at Cape Sable, Florida, and drive in a motor truck, through Florida, across Alabama, Mississippi and Louisiana, to El Paso, and back through Oklahoma and Arkansas. Dr. Edgar T. Wherry, of the Bureau of Chemistry, U. S. Department of Agriculture, will be a member of the party.

DR. WILLIAM RANDOLPH TAYLOR, assistant professor of botany in the University of Pennsylvania, will spend a part of the coming summer at the Dry Tortugas Laboratory continuing his study of the marine algae. The collections made last season revealed an unusually rich and varied flora, and developed a number of interesting ecological features.

DR. F. L. WASHBURN, professor of entomology at the University of Minnesota, is making preparations to head a second collecting expedition to the Tuamotu Archipelago and the Society Islands, in the South Pacific.

DR. B. M. DUGGAR, in charge of the graduate laboratory of the Missouri Botanical Garden, Washington University, St. Louis, lectured before the staff of the Ohio Agricultural Experiment Station, Wooster, on April 30 on "Some aspects of mosaic diseases."

DR. M. F. GUYER, head of the department of zoology in the University of Wisconsin, delivered the annual address before the joint meeting of Phi Beta Kappa and Sigma Xi in the University of Illinois on April 24. He spoke on "Some biological aspects of democracy."

JULIUS STONE, of Columbus, Ohio, delivered an illustrated lecture on "The canyons of the Green and Colorado Rivers" before the West Virginia Sigma Xi Club on May 1.

DR. T. WAYLAND VAUGHAN, director of the Scripps Institution for Oceanographical Research, delivered a series of five lectures on corals, foraminifera and limestones during the second week of April before the department of geology at Stanford University.

DURING March and April, Dr. D. T. MacDougal, director of the Desert Laboratory for Plant Physiology, Tucson, and Dr. B. L. Clarke, also of the Desert Laboratory, gave a series of five lectures before the Arizona section of the American Chemical Society, and the chemistry seminar of the University of Arizona. Dr. MacDougal's lecture dealt with "Physicochemical procedure in growth" and "Sap pressure and movement in plants." Dr. Clarke's lectures dealt with certain phases of his research at the Desert Laboratory and at Columbia University.

SPEAKERS at the Exposition of Inventions, held in New York City from April 27 to May 2, included Dr. Ira N. Hollis, president of the Worcester Polytechnic Institute; Professor Joseph W. Roe, of New York University; Elmer A. Sperry, inventor of the gyroscope, and Dean Dexter S. Kimball, of Cornell University.

DR. WILHELM KOLLE, professor of medicine in the University of Frankfort-on-the-Main, and director of the state institute of experimental therapeutics, ad-

dressed the scientific staff of the Rockefeller Institute for Medical Research, New York, April 17 on drugs.

THREE lectures on recent advances in the study of living cells were given at King's College, London, by Dr. Robert Chambers, professor of microscopic anatomy in Cornell University Medical School, on April 27, 28 and 29.

THE executive committee of the Dr. Andrew J. McCosh Memorial Association has turned over $200,000 to the Presbyterian Hospital, New York City. Half of it is to construct the Dr. Andrew J. McCosh operating amphitheater in the new Columbia-Presbyterian Medical Center and half to establish the Andrew J. McCosh Memorial Fund. Income from the memorial fund is to be used for the increase and diffusion of medical and surgical knowledge under the direction of the board of managers of the hospital.

THE deaths are announced of two Austrian men of science: Dr. Carl Ulrich, formerly director of the radium works at Joachimsthal, and Dr. Johann Palisa, director of the Observatory of the University of Vienna.

THE International Health Board has offered £1,000 a year for five years to establish a chair of tropical sanitation and hygiene at the Imperial College of Tropical Agriculture at St. Augustine, Trinidad.

ACCORDING to the *British Medical Journal*, Paris is preparing to celebrate the centenary of Jean-Martin Charcot, who was born in 1825 and died in 1893. In 1862 he became physician to the Salpêtrière Hospital, where he gradually built up the world-famous neurological clinic. The centenary organizing committee, under the patronage of the President of the French Republic and the leading statesmen and scientists of France, includes as honorary presidents Professors Pierre Marie, Albert Pitres and Paul Richer, who were pupils and colleagues of Charcot. The president of the committee is Professor Babinski. Early in June commemoration ceremonies will be held in Paris by the Academy of Medicine, the Faculty of Medicine and the Paris Society of Neurology. This society will hold at the same time its sixth international neurological reunion, celebrating the twenty-fifth anniversary of its foundation and the work of Charcot. Foreign governments, universities and scientific societies will be represented by delegates, and a large number of neurologists from many countries have signified their intention of joining in the celebrations.

THE London correspondent of the *Journal* of the American Medical Association reports that a good response has been made to an appeal for funds to establish a memorial to the late Sir William Macewen. It will be applied to three main purposes: 1. The provision of a bust to be presented to the Glasgow University and a replica for Lady Macewen. 2. The endowment of a Macewen memorial lectureship, to deal with some surgical subject, and to be given annually or biennially. 3. The foundation of a Macewen medal or prize in surgery, to be awarded annually. To achieve these three objects, the committee finds that $15,000 will be required. The amount so far obtained is $7,750. Among foreign subscribers are: Professor Harvey Cushing, of Baltimore; Professor Rovsing, of Copenhagen; Professor Hartmann, of Paris; Dr. W. W. Keen, of Philadelphia; Professor Alessandri, of Rome; Dr. George W. Crile, of Cleveland, and Professor de Quervain, of Berne.

ACCORDING to a press dispatch, J. T. Scopes, science teacher in Rhea High School, Dayton, Tenn., was arrested on May 5 on a charge of violating the new Tennessee law prohibiting the teaching of evolution in the state public schools.

JOHN R. FREEMAN, consulting engineer, Providence, has made a gift of securities valued at $25,000 to the Boston Society of Civil Engineers for the establishment of a fund, the income of which is to be used for encouraging research by the younger engineers of the society through the establishment of prizes for papers on hydraulics and allied subjects.

THE American Museum of Natural History has been granted the use of about forty acres of mixed forest and open land in the Palisades Interstate Park on which to establish a station for the study of insects. This tract is in the mountains near Tuxedo, New York. The station will furnish an excellent opportunity for technical research but its chief function will be educational. Living insects will be on exhibition during the summer season. These exhibits will be fully labeled and, in addition, docent service will be provided for the more interested visitors. Dr. Frank E. Lutz is in charge.

THE 1925 New York State Legislature has appropriated a sum of $20,500 for special investigations with canning crops at the State Agricultural Experiment Station at Geneva. The bill provides for the appointment of three new associates in research, one each in plant pathology, entomology and vegetable gardening. The investigations will be carried on on a 60-acre farm in the vicinity of Geneva, and work will begin on the project on July 1. The U. S. Department of Agriculture has established a permanent substation at the station for the study of the Mexican bean beetle which is expected to invade the

state in 1926. Isolated specimens of the pest have already been found in the western part of the state.

THE French Chamber of Deputies has provided for a tax of 5 centimes on each 100 francs paid in salaries by French commerce and industry. The products of the tax, which it is estimated will bring in about fourteen million francs a year, is to be allocated to French scientific laboratories.

THE collection of astronomical and mathematical instruments, dating back to the tenth century, and illustrating the early history of the sciences, which has been presented by Mr. Lewis Evans to Oxford University and housed in the Old Ashmolean building, was opened for public exhibition on May 5 by Lord Crawford. The degree of honorary doctor of science was conferred on Mr. Evans. A volume containing the *Transactions* of the Oxford Philosophical Society from 1683 to 1690, transcribed from the minute-book long preserved in the Ashmolean Museum, was published on the same date. The editor was Mr. R. T. Gunther, of Magdalen College, known for his researches in the early history of science.

THE Vienna Exposition of Hygiene was opened on April 28 by President Hainisch. The exhibition is divided into two parts—"the human body" and "the modern household." Austria and Germany cooperated in the work of establishing it, the well-known Dresden exhibit of hygiene forming its basis. Among interesting features are demonstrations by institutions for preventing tuberculosis and the treatment of chronic drunkards. The city of Vienna occupies a large space for showing modern institutions for social welfare, especially model tenements erected at the cost of the municipality of Vienna to solve the housing problem, as also land settlements outside of Vienna. These model tenements are said to surpass tenements anywhere in Europe or the United States in modern construction, labor-saving devices and space areas.

UNIVERSITY AND EDUCATIONAL NOTES

HARVARD UNIVERSITY has received a gift of $125,000 from Harold S. Vanderbilt to provide a gymnasium for students in the medical school. Provision is also made for payment of the salary of a competent gymnasium instructor during the next five years.

AS a memorial to the late Marion Leroy Burton, president of the University of Michigan, $100,000 has been collected, the income of which will be paid to Mrs. Burton during her life time and to her children until they reach the age of thirty years. Thereafter the fund will revert to the university.

SAMUEL INSULL, president of the Edison Commonwealth Company of Chicago, has made a gift of $25,000 to Union College at Schenectady.

A LAW becomes effective on July 1 creating a board of regents, which has the authority and duties formerly conferred on the state board of administration, relating to the University of Kansas, Kansas State Agricultural College and its experiment stations and the Kansas State teachers colleges of Emporia, Hays and Pittsburg. The board will be composed of nine members appointed by the governor for a term of four years.

AT Vanderbilt University, Dr. Walter E. Garrey, professor of physiology at Tulane University, has been appointed professor of physiology; Dr. Barney Brooks, associate professor of clinical surgery at Washington University, St. Louis, professor of surgery; Dr. Horton Casparis, associate in pediatrics, the Johns Hopkins Medical School, associate professor of pediatrics; Dr. Francis A. Swett, instructor in anatomy at the Johns Hopkins Medical School, associate professor of anatomy; Dr. Arthur W. Wright, of the Boston City Hospital, assistant professor of pathology.

DR. EDWARD F. MALONE, professor of histology at the University of Cincinnati, has been appointed professor of anatomy in the medical college to succeed Dr. Henry McE. Knower, who recently resigned.

PROFESSOR EMIL F. GUBA, of the department of plant pathology of Cornell University, has been appointed assistant research professor in botany at the Massachusetts Agricultural Experiment Station at Amherst.

DR. GEORGE R. HARRISON, who received his college and university degrees from Stanford University, has been appointed assistant professor in the department of physics. He is at present a National Research Council fellow. He was graduated in 1919, became master of arts the next year and doctor of philosophy in 1922.

DISCUSSION AND CORRESPONDENCE

THE ETHER DRIFT

THE recent important announcement by Professor Miller of the detection of an ether drift on the summit of Mt. Wilson makes it advisable for us to look carefully over the whole subject in its proper perspective.

The theory of relativity was not founded wholly upon the negative result of the Michelson-Morley experiment. There were other similar experiments with negative results and of equal importance, though not so generally known. Such were the experiments of Trouton and Noble, who looked for the production of a torque upon a suspended charged condenser; the experiments of Rayleigh and of Brace, who failed to find any trace of double refraction in transparent moving bodies, and certain experiments designed to

test the electrical conductivity of conductors in different orientations with respect to the earth's motion in space. True, all these experiments were performed at or near sea level; and in view of the difference in Professor Miller's results as between Cleveland and Mt. Wilson, it is certainly desirable that some or all of these other experiments should be performed at as great an altitude as possible; for no final conclusion in the matter can be reached until all the different lines of experiment give concordant results.

Of these other lines of experiment, the simplest and most practical would seem to be those of Rayleigh and of Brace (*Phil. Mag.*, December, 1902, p. 678; *ibid.*, vol. 7, 1904, p. 317). It ought to be possible to test this point with an apparatus compact enough to be carried up to a height in an airplane.

The Trouton-Noble experiment is still so uncertain in theory that we do not know exactly what to look for. (Kennard, *Bulletin* of the National Research Council, vol. 4, part 6, December, 1922, No. 24.) Its experimental prosecution may well be delayed until its theory is clear.

PAUL R. HEYL

BUREAU OF STANDARDS

THE DISAPPEARANCE OF HELIUM IN GEISSLER TUBES

THE recent comment by S. C. Lind and D. C. Bardwell in the March 27 number of SCIENCE, entitled "Mercury and Ionized Helium," in which experiments are described which appear to show that helium and mercury do not interact as a result of alpha particle bombardment, suggests our calling attention to some interesting phenomena associated with the disappearance of helium in Geissler tube discharges which produce the spectra of the first negative Deslandres group of carbon, and the so-called comet-tail bands.

Using tubes of the Wood type of four millimeters bore with pressures of twenty millimeters of helium and a small partial pressure of 10^{-4} mm of residual compounds of carbon from activated charcoal, on several occasions during energetic excitation of the above mentioned spectra, together with the brighter lines of helium, the latter has been observed entirely to disappear. The tube changes in color from pink to blue, and the Ängstrom bands and triplet band system, described by Merton and Johnson, are developed. The disappearance of helium in its usual form under these conditions is inferred from the complete disappearance of its spectrum and a marked reduction of gas pressure within the tube, as exhibited by the length of the dark space.

Conditions hardly permit of the hypothesis of occlusion in the ordinary sense, since subsequent baking the tube to the softening point of pyrex glass does not recover the helium spectrum. Neither can one ascribe the phenomenon to diffusion through the glass walls, since the tube is not heated much above 50° C. by the discharge which causes the extinction of the helium spectrum. One is reminded of some early work of Berthelot, *Ann. Chem. Phys.* (VII), 11, p. 219, 1897, on the apparent disappearance of helium when associated with carbon compounds.

HARVEY B. LEMON

RYERSON PHYSICAL LABORATORY,
UNIVERSITY OF CHICAGO

THE CLASSIFIED CONTINUATION CARD CATALOGUE OF THE BIBLIOGRAPHY OF FISHES

IN the *Anatomical Record* of December 25, 1924, Vol. 29, pp. 128–129, I published under the above title a preliminary abstract of a paper which I read on January 1, 1925, before a joint meeting of the American Society of Zoologists and the American Ecological Society. The purpose in publishing this abstract and in reading this paper was to give notice that the "Bibliography of Fishes" is being kept up to date, to ask "fish men" to send in their articles for immediate carding and to ask all interested in any subject wherein fishes touch the life of man to come to the American Museum to get their references brought as near to the actual date as is humanly possible. In order to secure the widest possible notification of this purpose to scientific men in America I am publishing in SCIENCE this note on the plans and work now under way.

A large number of the letters and reviews received since the publication of the "Bibliography of Fishes" have expressed the strong hope that the bibliography, which includes the literature to and ending with 1914, might be continued. My own feeling is that having learned how to do bibliographical work, I owe it to science in general and to ichthyology in particular to keep the "Bibliography of Fishes" up to date in the form of a classified card catalogue. For a year and a half this work has been carried on in tentative fashion and it has been demonstrated that it can be done without any outside assistance and without any outside expense to the museum—*i.e.*, it is being done as a part of the regular routine work of the department.

The sources of additional reference for the "Bibliography of Fishes" are primarily those papers which have appeared since the close of the bibliography (1914). These references come in to us in three separate ways: First, in the current bibliographies which generally appear about two years later than the literature which they record. Of these the "Zoological Record" has been checked up to date, and I plan shortly to begin on the "Archiv für Naturgeschichte." When that is done the continuation (post 1914) of the "Royal Society Catalogue," the "Concilium Bibliographicum" and other like annual works will all be

checked against our alphabetical card catalogue, the missing cards put in it and duplicates made and distributed in the index-catalogue.

In the second place bibliographies in recent books and in extensive papers dealing with particular subjects or structures will be checked, since in such sources are found many valuable references old and new which do not get into the annual bibliographies. In this way we expect to get some 25 or 30 per cent. more titles than are to be found in the annual issues of the "Zoological Record."

In the third place we receive here a considerable number of separates, which are checked, carded, indexed and the cards distributed as soon as they come in, and thus our catalogue is about two years ahead of the "Zoological Record" and the other annual bibliographies. The greater the number of such separates which come in, the more nearly up to date our card catalogue will be, and this is the point in this appeal for the early sending in of such separates. These references once carded become available for all who may call at the museum.

Now readers of this article are already asking, "What about publication of this card catalogue?" This is not a question that I can answer. It will be years before material will be accumulated in sufficient amount for another volume. Whenever that time comes, it will be for the president and trustees of the museum to decide the question of publication.

The results of my work can not be sent out by letter, but to any one calling at the department the recent references in his particular subject will be put before him on five minutes notice. And such use, it may be remarked parenthetically, is being made of our catalogue at intervals nowadays.

Readers of SCIENCE *who are students of fishes, will you send me your separates promptly that they may promptly be made available for others? I want everything wherein fishes touch the life of man.*

E. W. GUDGER

AMERICAN MUSEUM OF NATURAL HISTORY,
NEW YORK CITY

SPECIAL ARTICLES

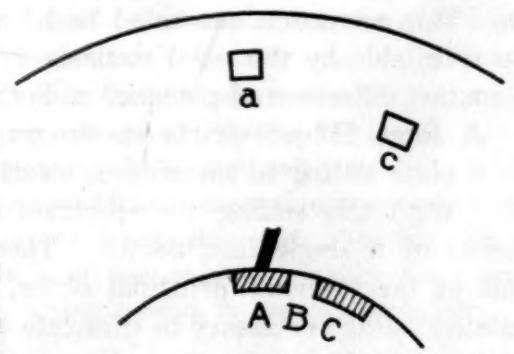
THE LIFE OF METASTABLE HELIUM AND MERCURY

KANNENSTINE (*The Astrophysical Journal,* 55, 345 (1922); 59, 108 (1923)) and Marshall (*Astrophysical Journal,* 60, 243 (1924)) have reported that in mercury and helium which has just been excited by a discharge, it is temporarily possible to cause an arc to strike at a very low voltage. This voltage is said to be critical and equal to the difference between the ionizing potential of the gas and its resonance potential; *i.e.,* 24.6 − 19.8 = 3.8 volts for helium, and 10.4 − 4.7 = 5.4 volts for mercury. Their interpretation of these results is that a large supply of excited atoms is formed during the primary arc, whose reversion to normal is impossible except by collisions of the second kind *with impurities* or with the walls and electrodes. The length of time during which it is possible to maintain these post-arc currents with low voltages is taken as a measure of the life of the metastable excited atoms.

These results were predicted by Franck and Reiche (*Zeitschrift für Physik,* 1, 154 (1920)) from considerations which are rendered doubtful by recent work on collisions of the second kind.

The author has therefore repeated the work of Kannenstine and Marshall, using more sensitive methods. A commutator was used which applied a high voltage (30–40 v.) to an arc tube for a short time (Period A). This voltage was then disconnected for an interval (Period B) which was long enough to allow all transient phenomena due to inductance and capacitance to die out. During a third interval (Period C) a second known voltage, V, was applied through a microammeter. This meter thus read the time average of a current which was zero except during Period C. It was then found that when V was very low (+1 volt) or negative, the current during Period C was negative and small (10–20 microamperes). As V increased from 1–6 volts, the current increased continuously and reached a definite saturation value of 4–5 milliamperes at 8 volts. No further increase was detected until V = 20 volts. The negative currents, together with the *complete absence of any critical potential,* led to the hypothesis that these post-arc currents are due, not to ionization of excited atoms, but to the persistence of actual ions in the arc space.

To test this hypothesis, a perforated disc was mounted on the same shaft as the commutator. The arrangement is shown schematically in the figure. During a portion of Period A, the arc is visible



through the opening *a*. Similarly, during part of Period C, it is visible through *c*. Experimental conditions were then adjusted so that the current flowing during Period C was 1/4 to 1/3 that flowing during Period A. It was then found that though intense

spectral emission was observable during Period A (*i.e.*, through opening *a*), there was no detectable emission during Period C. Since it is known from various lines of evidence that spectral emission is not appreciably the result of recombination of ions and electrons, but is the result of excitation, this proves conclusively that the post-arc currents are due to the neutralization of negative space charge by the persisting positive ions, and not to the ionization of excited atoms.

Furthermore, Kannenstine's results using a Braun tube were reproduced in all essential details, in very pure helium, in helium of ordinary purity (no impurity visible spectroscopically) and also in helium containing large amounts of mercury vapor. No quantitative difference greater than the limit of reproductibility under the same conditions was found.

Many of the peculiarities of the Braun tube figures were found to be produced by the resistance of the potentiometer and other elements of the circuit.

CARL ECKART

PRINCETON UNIVERSITY

THE SUCCESSIVE STIMULATION OF THE ARC LINES OF HELIUM BELOW THE IONIZATION POTENTIAL

RECENT experiments made in the research section of this laboratory have demonstrated photographically the successive stimulation of the arc lines of helium under increasing potential of electron bombardment between the resonance and ionization points.

The potentials necessary for the stimulation of the lines of the visible spectrum were calculated from the established term values of these lines and from a term value for the normal atom based on the assumption that the correct ionization point is 24.5 volts. The photographs show a concordance of observation with calculation to better than one tenth of a volt. The voltmeter corrections were made from ionization point curves taken under the same conditions in the same apparatus assuming the correct point to be at 24.5 volts. This correction amounted to 1.1 volts, a value also calculable by the usual methods by application of contact difference of potential and other corrections. A large Hilger quartz spectrograph was used with a plate setting to cover from about 5000A to 3000A. With this setting the spectrum at 22.9 volts consists of a single line, 3889A. This is the second line of the coplanar principal series, m = 2. The calculated voltage necessary to stimulate this line is 22.9 volts. At 23.5 volts three lines are found 5016 (calculated 23.00 volts), 4713 (23.51) and 3889. At 23.9 volts nine lines occur, 5048 (23.56), 5016, 4922 (23.65), 4713, 4472 (23.65), 4121 (23.88), 3965 (23.65), 3889 and 3188 (23.62). At 25 volts the full spectrum is obtained.

When viewed visually the lines appear and extinguish sharply in order. The disappearance of the line 5048 leaving 5016 adjacent to it furnishes a convenient method for establishing instrumental corrections if the calculated value for the appearance of the line is accepted as correct.

The experiments are an extension of work reported to the American Physical Society at its Washington meeting in April, 1924. The apparatus described at that time has been altered by the introduction of an equipotential lime cathode of a type similar to that suggested by G. Hertz (*Zeit. für Phys.*, 22, 24, 1924), but of a different shape, sharply peaked at its narrowed center to permit closer approach to the grid. The distance of electron acceleration did not exceed one half of a millimeter. Under these conditions both voltage and current were remarkably steady. No "kicks" or "hysteresis loops" were found in the current-voltage curves nor were there any evidences of oscillatory disturbances.

These results are a confirmation of the early observations of Rau (*Sitz. Ber. d. Phys. Med. Ges.*, Wurzburg, 1914) and of Richardson and Bazzoni (*Nature*, 98, 5, 1916) and of the recent publications of G. Hertz (*l. c.*).

C. B. BAZZONI,
J. T. LAY

RANDAL MORGAN LABORATORY,
UNIVERSITY OF PENNSYLVANIA,
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Measurement of the circulation in man: DR. YANDELL HENDERSON and HOWARD W. HAGGARD, Sheffield Scientific School, Yale University. Ever since Harvey showed that the blood circulates, the determination of the volume of flow per minute has remained the outstanding, unsolved problem of the circulation. It is not merely general knowledge that is needed, but rather the means of determining the functional efficiency of the circulation in individuals in all conditions; in other words, a simple and fairly accurate method for measuring the circulation in man. The method based on absorption of nitrous oxide from the lungs is inaccurate and of limited applicability.

Investigations in this laboratory have led to the formulation of the principles controlling the absorption of any gas whatever by mere solution from the lung air into the blood. These principles show that the rate of absorption of a very soluble gas, such as ether or alcohol vapor, is dependent mainly on the volume of air breathed, that is, the respiration; while the rate

of absorption of a relatively slightly soluble gas is more nearly proportional to the volume of the blood stream through the lungs, that is, the circulation. Gases of about the proper solubility were therefore tried, one after another, until the vapor of ethyl iodide was tested. With this substance a simple, accurate and reliable method for measuring the circulation in man has been developed. It is applicable to practically all conditions.

A complete determination of the respiratory exchange, with the oxygen consumption as a basis for indirect calorimetry, is made with each measurement of the circulation. The whole process of inhalation, and the analyses for oxygen, CO_2 and ethyl iodide can be completed by one operator in half an hour.

Results have been obtained on a variety of persons from athletes to cardiac patients. For purposes of comparison and estimation of efficiency, the most significant features of the data reported are the stroke index, or volume in milliliters of blood per kilo body weight discharged by the heart at a beat, and the arterio-venous oxygen difference. The stroke index for normal persons during sitting rest ranges from 1.3 to 1.8. In some individuals it is much larger in the recumbent position —up to 2.0 or more; and much smaller in the standing position—down to 1.0 or less; these observations confirm those of Bock and Field. During vigorous exercise the stroke index may rise to about 3.5.

The arterio-venous oxygen difference in normal persons during rest is about 4 volumes per cent. This means that the circulation is normally so large in relation to the oxygen consumption of the body that the venous blood returning to the right heart contains on the average only 4 volumes per cent. less oxygen than the arterial blood. The blood stream pumped by the heart is thus about twice as large as was formerly supposed. During bodily rest its volume in liters per minute is about equal to the volume of air breathed per minute. During exercise respiration may increase 8 or 10 fold, and the circulation 4 or 5 fold.

Hot baths quicken the circulation without greatly increasing the oxygen consumption. Thus they further reduce the oxygen difference and raise the pressure of oxygen in the tissues. On the other hand during muscular exertion, which is vigorous but such as can be sustained for a considerable time, involving an oxygen consumption of about 2 liters per minute, the circulation increases from a resting value of 7 or 8 liters per minute for a man of 70 or 80 kilos, up to a flow of 30 liters or more per minute. Under such conditions the arterio-venous oxygen difference also increases, reaching 8 volumes per cent., or 40 per cent. utilization, or more. During greater exertion the oxygen difference increases, because the circulation does not.

In cardiac patients the inefficiency of the circulation shows itself in the increase of the arterio-venous oxygen difference above the normal value. The capacity of the circulation to keep the oxygen difference down, and thus to keep the pressure of oxygen in the tissues up, appears to be the limiting factor in the maximum power that can be sustained by athletes at one end of the scale, and by cardiac patients at the other.

Cell respiration: DR. F. G. NOVY and DR. M. H. SOULE, University of Michigan. This paper presents some of the results obtained in investigations on the respiration of micro-organisms. A compensation manometer has made it possible to observe the pressure changes which take place within a culture tube or jar, and to remove samples of the contained gas for analysis. The gas exchange of an organism can thus be followed, whether grown in air, or in varying concentrations of oxygen, carbonic acid or other gases.

Cylinders of sterile raw potato consume all of the oxygen in a tube and return a corresponding amount of carbonic acid, the respiratory quotient being 1.0. When the oxygen is consumed, or if it is replaced by nitrogen, the potato respires as an anaerobe. It is known that anaerobic bacteria will grow, apparently in the presence of air, provided a piece of raw potato is added to the broth. In such case, the favoring action of the potato is due to the removal of the dissolved oxygen.

The study of the respiration of certain pathogenic protozoa, Trypanosomes and Leishmanias, showed that they required oxygen and that the gas exchange was much the same as in the case of bacteria. When grown in tubes, with an air capacity of about 100 cc., all of the oxygen was removed in 6 to 10 days and more than 15 per cent. of carbon dioxide was returned. When cultivated on blood agar, the respiratory quotient was found to average 0.82 to 0.87; in the presence of glucose, the quotients were 0.94–0.95. Increased concentrations of oxygen inhibited the growth. Carbon dioxide, in 20–30 per cent. concentration, was also inhibitive.

The average respiratory quotient of the human tubercle bacillus, when grown on glycerine agar, was 0.84; on glucose agar, 0.99. The bovine bacillus gave slightly higher values. These results closely approximate theory and show that the glycerine and glucose are utilized as sources of energy for the growth of these organisms.

When grown on glycerine agar, a single tube culture can consume all of the oxygen in 700–1000 cc. of air. On glucose agar the oxygen consumption is less. The human type can be grown in concentrations of oxygen up to 100 per cent., whereas the bovine type is inhibited by 80 per cent. Carbon dioxide, in excess of 60 per cent., shows some inhibition.

The tubercle bacillus must have oxygen, but it can grow in an atmosphere which contains 0.5 per cent. or less of oxygen. The slow and poor growth, obtained under these conditions, is not due to lack of oxygen but to the low tension of this gas. This fact explains the slow growth of the organism within the human body since the oxygen tension in the tissues is low.

The anti-sterility vitamin, fat soluble E: HERBERT M. EVANS and GEORGE O. BURR, University of California. When rats are reared on various "synthetic" food mixtures consisting of fat, carbohydrate and protein in separate and relatively pure form together with an appropriate salt mixture and the vitamins A and B, they grow well and have every appearance of health.

Depending somewhat on the exact character and the proportions of the constituents of the food, they sooner or later exhibit complete sterility. The sterility is a dietary deficiency disease for it can be cured or prevented by a change in dietary régime, a change involving the addition of certain single natural foods high in a new food factor or the addition of very much smaller amounts of extracts of those foods.

The sterility disease affects males and females differently. In the male it eventually leads to destruction of the germ cells (eventually the entire seminiferous epithelium) but this is not the case with the female, where the ovary and ovulation are unimpaired throughout life but where a highly characteristic disturbance occurs in gestation, the death and resorption of the developing young. It is necessary to insist on the peculiar character of dietary sterility, thus produced in the female through lack of vitamin E, for it is only by ascertaining the existence of typical "resorption gestations" that one may be assured that he is dealing with deficiency in the specific substance E. Many other dietary delinquencies cause sterility in the female, but they all do so by interference with other steps in the reproductive mechanism than those involved in lack of E, usually by preventing oestrus, ovulation, fertilization or implantation but not by resorption after implantation has occurred.

In gestations where E is low or absent, the embryos seem at first normal, but sooner or later, often by the eighth day, retardation in development can be substantiated. Evident abnormality, especially monstrosity, does not occur. At some time between the twelfth and twentieth day, foetal death occurs.

Large numbers of females have been reared on various "pure" food régimes and bred shortly after the sixtieth day of life. Only those exhibiting a typical resorption were now employed to trace the distribution and abundance of the new food factor E in natural foods. Shortly after the failed or resorption gestation a small amount of a single natural food stuff was now added to the ration or fed separately from it and the fate of the next gestation followed with similar care. In many instances a normal sized litter of vigorous young resulted. In others, no alteration of the sterility was secured. We have thus charted the considerable and inconsiderable possession of E on the part of common foods. It is present but never highly concentrated in a great variety of animal tissues, musculature, fat and viscera, included in the latter being pancreas, spleen, liver, heart, hypophysis and placenta. One of the most remarkable things about the content of E in animal tissues is the fact that the vitamin is low in the viscera. It is lower in the liver than in the musculature. A daily feeding of half the total liver of rats reared on natural foods will not invoke fertility. There is failure also when the entire heart, spleen, brain, kidney or testes are fed daily. The musculature and fat, on the other hand, while not a concentrated source of E, contain in their totality several times the minimum requirement for a successful gestation. E is present but extremely low in milk fat.

Cod liver oil, though high in vitamins A and D, is notably lacking in E. Throughout the life of animals, 9 per cent. by weight of the ration may be constituted by cod liver oil, a single drop of which daily is adequate for A requirements and yet sterility results. In contrast with the paucity of E, even in its most abundant depots in animal tissues, is its concentration in the organs of certain plants, especially in seeds and green leaves. It can be demonstrated to be unhurt after careful desiccation of such leaves (lettuce, alfalfa, pea, tea). Thus in a series of experiments, one and one half, one half and finally one fourth gram daily of the lettuce leaf powder proved efficacious in cures. E is high in some cereals. We have found it in oats, corn and especially wheat, where it is low in the endosperm, but concentrated in the embryo. The richness of wheat germ in E is extraordinary. We have found no other naturally desiccated substance comparable to it in value; 250 mg. daily evokes cures. In the case of both wheat germ and lettuce leaf, ether extraction of the carefully desiccated substance removes E quantitatively and secures for us oils which are efficacious in *daily*, single drop (25 mg.) administrations. E is probably present in most commercial oils so that when the latter constitute a high proportion of the diet, for instance, when fed as 15 per cent., displacing lard, fertility results.

We have completed a series of cannibal experiments. Sterile females reared upon "pure" food régimes were sacrificed daily and their tissues (liver, musculature and fat) fed to other females reared in an identical fashion and likewise of proven sterility. At the same time, normal females of proved fertility were similarly sacrificed and fed to other sterile "pure" food females. In all instances, the tissues of rats reared on a natural food régime were able to invoke fertility in their sterile sisters. Of even greater significance would seem the demonstration that in no instance could a cure be obtained by the administration of the same tissues from sterile females.

If animals are reared on a diet of natural food stuffs and after their fertility is established, shifted to a pure food ration, they preserve their fertility for three or four months, when they lose it. Similarly, when sterile animals are cured with foods possessing the new vitamin, not only is the next gestation normal, but in some circumstances, the next two or three gestations.

Vitamin E is transferred from mother to offspring during intra-uterine life, for the tissue of new born rats acts as a cure of female dietary sterility.

An excess of E can not increase fertility beyond normal limits. The administration to sterile animals of foods or extracts of foods known to be twice to twenty times as rich in vitamin E as is required for the birth of living young does not increase litter size or weight or in other ways improve the performance of the reproductive mechanism beyond the normal limits.

The symbiosis between termites and their intestinal protozoa, and the toxicity of oxygen for protozoa: DR. L. R. CLEVELAND, National Research Fellow, at the Johns Hopkins University. The intestines of all wood-feeding termites are teeming with countless millions of large

flagellate protozoa, which digest wood for themselves and for their hosts, the termites in which they live, move and have their being. This protozoal horde may be removed from the termites by incubation, by starvation and by oxygenation, and neither method of removing the protozoa kills the termites, but they are not able to live more than three to four weeks after their protozoa have been taken from them. Give the protozoa back, and they will live indefinitely. The protozoa can not live outside of termites. The termite intestine is their gastronomical paradise. Here, then, is an ideal partnership; it is imposible for one partner to live without the other.

Cockroaches harbor two kinds of ciliate and two kinds of flagellate protozoa, all of which may be removed by confining the cockroaches in oxygen, and the confinement does not injure them at all. But cockroaches, unlike termites, are able to live indefinitely after their protozoa are taken from them. The protozoa of many other, probably all, insects may be removed by oxygenation. Oxygenation, then, will be of great value in working out the exact rôle that insects play in the transmission of protozoa from man to man, from animal to man, from animal to animal, from plant to plant, and from plant to animal.

All the protozoa of earthworms, salamanders and fish may be removed by oxygenation, and without injury to the earthworms, salamanders and fish. There are many protozoan diseases of fish which oxygenation will very probably cure.

Frogs harbor many different kinds of intestinal protozoa (often seven different genera are present in a single host), all of which are killed when the frogs are confined in oxygen, and, as in the case of other animals, without injury to the frogs.

It is highly probable that all intestinal protozoa may be removed from all invertebrates and from all cold-blooded vertebrates by oxygenation, and that none of these hosts will be injured. Pure oxygen is toxic for all forms of life and especially so under pressure, but at 3.5 atmospheres it is very much more toxic for the protozoa of these animals (invertebrates and cold-blooded vertebrates) than for the animals themselves; in some instances the protozoa are all killed in less than one hundredth the time required to kill the animal in which they live. In other words, oxygen is more than one hundred times as toxic for the parasites as for the hosts. Why is this true? It is very probably due to the fact that the protozoa have acclimatized themselves to an environment with a small amount of oxygen, very much smaller than that of their host, and when placed in an environment (*i.e.*, when oxygenated) with much more oxygen they are unable to stand the change, because the change for them is far greater proportionally than for their host, and they die, while their host is unaffected.

Such a physiological difference between hosts and parasites is taken advantage of here for the first time to free animals of their protozoan parasites, and it may be possible to take advantage of it to a still greater extent and free animals and plants of other parasites, inclusions, bodies and agents by oxygenation.

Some concurrent changes involving the newer aspect of the sex problem: DR. OSCAR RIDDLE, Carnegie Institution's Department of Genetics. Investigations conducted during recent years provide a considerable body of data—obtained particularly on frogs and birds—which indicate that the normal chromosomal control of sex may be completely reversed by special conditions; and that the several special means employed to effect such sex-reversal really become effective through pronounced and prolonged changes of metabolic rate in the developing organism. This is designated the "newer aspect" of the sex problem; the facts concerning the usual or normal control of sex by the sex chromosome mechanism may be regarded as the older and now well-established aspect of the sex problem. Facts of the latter kind lie wholly within the field of genetics; but the available data on the newer aspect of sex are principally facts of biochemistry, physiology and embryology, and only in small part are they genetic data. One object of this presentation is to supply evidence that a newer aspect of the sex problem now exists.

If metabolic distinctions of sex are even more fundamental than are chromosomal differences it might be possible, in specially suitable material, to associate seasonal changes in sex ratios and other sex phenomena—as these have earlier been found in pigeons—with parallel seasonal changes in the metabolism of the parent animals. Besides the nervous system the organs now believed to be chiefly responsible for effecting variations in metabolic rate are the thyroids and suprarenals. Data showing the monthly and seasonal size changes in the thyroids of three kinds of pigeons have now been obtained. Curves constructed from these data show that the thyroids are notably larger during autumn and winter and become reduced in spring and are lowest in summer. Three kinds of sex data previously obtained—and earlier partially described as indicating a relation of metabolic rate to sex—have now been classified according to the seasonal changes in thyroid size. This grouping of those earlier results shows:

(1) As obtained under the breeding methods practiced by us the sex ratio varies with the seasonal fluctuations in thyroid size. The highest proportion of males corresponds to the period of largest thyroids; the highest proportion of females to the period of small or smallest thyroid size. (2) Seasonal changes in the storage metabolism within pigeon ova, as measured by burning the yolks in a bomb calorimeter, also parallel the changes in thyroid size. Least storage is found in winter; most in summer and early autumn. (3) A small seasonal difference is found for the percentage of alcohol-soluble material stored in the ova of pigeons. The lower percentage of fat corresponds to periods of larger thyroid size; higher storage values are associated with smaller thyroid size.

Concurrent changes are thus found in four kinds of data concerning metabolism on the one hand and sex on the other. Some deviations from the above stated rules are found, these chiefly and rather consistently concern the autumn period. It is suggested that this discrepancy may result from the prolonged hypertrophy of

the suprarenals which has earlier been shown to accompany continuous egg-production in pigeons. It is not considered probable, however, that the seasonal fluctuations of thyroid size which doubtless occur in higher animals generally have any influence on the sex ratio under normal conditions. These results have been observed only under special experimental conditions, and even here the relation of thyroid size of parent to sexuality in the offspring is not regarded as simple or direct.

These data demonstrate the practicability of dealing with the sex problem by biochemical and physiological methods. They bring the results obtained on the pigeon and the frog into clearer harmony—Adler having ascribed a male-determining action to the strongly acting thyroid in tadpoles arising from "over-ripe" eggs. The results appear to provide new information on the intimate nature of sex. They also probably permit a closer view of the way in which the sex chromosomes perform their normal function.

Experimental chemical and bacteriological pollution of wells via the ground water route: PROFESSOR C. W. STILES, U. S. Public Health Service. In the prevention of hookworm disease, excreta-disposal is of fundamental importance, especially in connection with the possibility of infecting water supplies. The views sanitarians have held on this subject have been so divergent that an extensive series of experiments were undertaken in order to uncover the underlying principles involved. The experimental field, with about 500 experimental wells, is isolated far from habitations so that the danger of spreading infection has been eliminated. In these experiments, pollution, both chemical and bacterial, was placed in the ground. The chemical pollution has been recovered in wells up to a distance of 414 feet, and the bacterial infection 232 feet, away from the point it was placed in the ground. These results effectively dispose of the claim that bacteria can be carried only a few feet under ground. It has been proved that the pollution extends only in one direction, namely, in the direction of the ground water flow; it floats in a shallow band at the top of the ground water table. During dry weather, when the water level falls, the bacteria tend to filter out into the soil; during wet weather the water level rises, picks up the bacteria, and carries them further; if dry weather continues sufficiently long, the bacteria die. Under favorable conditions, however, the bacteria have lived under ground up to 2 years and 8 months, and chemical pollution put in the ground 2 years and 9 months ago is still being recovered from the wells. Thus, the rise of the ground water, due to rain fall, results in spreading underground pollution to the wells while the fall of ground water, due to continued dry weather, results in purifying the ground water supply. The principles uncovered by this investigation have their practical application in sanitation, not only as respects the distance but also the location of wells from possible sources of pollution. Further they show that the circular type of sanitary devices is safer than the almost universally adopted quadrangular type.

A banana in the Tertiary of South America: EDWARD W. BERRY, The Johns Hopkins University. The original home of the banana (Musa) has always been a disputed problem, most authors inclining to the belief that it was in the southeastern Asiatic region, since there are so many cultivated varieties as well as wild forms in that region at the present time, and since the equivalent word occurs in the Sanscrit, Arabic, etc.

More particularly botanists have, almost without exception, disputed that the genus Musa was indigenous in the Western Hemisphere, or was cultivated here prior to its introduction into Hispaniola from the Canary Islands in 1516, as related by Oviedo in 1556.

Alexander von Humbolt was the first to question this belief and to maintain that the banana was probably a native of America in pre-Spanish times, and in recent years O. F. Cook has argued that it was an aboriginal American crop-plant, originally cultivated as a root crop, the valuable pulpy fruits, normally seedless, being a result of hybridization, since the so-called wild seed-bearing species have non-edible fruits more or less similar to the capsular fruits of all the other genera of the family.

I have received the fossil seeds of a species of Musa from Dr. Maurice A. Rollot, who collected them from the coal measures of the Cerros de Guadalupe and Montserrate which form a part of the eastern upland border of the Sabana of Bogota in Colombia. The altitude is about 9,000 feet and the geological horizon is probably Oligocene, although there is some doubt as to its precise age.

These more or less compressed and highly lignified fossil seeds are larger than those of any of the existing Malaysian species known to me, but may be matched by the seeds of several existing African species, among which they most resemble those of the so-called Abyssinian banana, *Musa ensete* Gmelin.

Banana-like leaves, described as Musophyllum by paleobotanists, have long been known from the Tertiary of the United States, Europe, Central and South America, but it is altogether impossible to distinguish the leaves of Musa from those of the exclusively American genus Heliconia. In the case of the seeds, however, this uncertainty disappears, since the seeds of Musa are perfectly characteristic and strikingly different from those of all the other genera of the family.

The present discovery shows that Musa was a member of American Tertiary floras and although it does not necessarily prove that the banana was brought into cultivation by the aborigines of the western hemisphere, it lends probability to such a belief, and in a measure serves to substantiate statements to this effect made by Garcilaso de la Vega and Montesinos in the early part of the sixteenth century.

Outlines of the history of the recent fauna of Palearctic Asia: PROFESSOR PETER P. SUSHKIN, Russian Academy of Sciences. I wish to thank the local committee of the academy for the opportunity of presenting to you my views concerning the history of the recent fauna of Siberia and High Asia. These views are the result of studies based in part on my field work in the

Kirghiz Steppe, in southern Siberia, and in N. W. Mongolia. At present, most zoogeographers adopt the divisions of Palearctic Asia into two subregions; the northern, which extends across the Eurasian continent from the Pacific to the Atlantic, and the more southern, or High Asian.

Starting with the study of birds, I have found that nearly one half of the fauna of every locality in Palearctic Asia consists of species which may be termed Transpalearctic, since they extend from ocean to ocean: some of them are also widely distributed from north to south, whereas others are restricted to one of the zonal provinces. The remainder consists of groups of more restricted distribution. For Siberia, leaving out the Transpalearctic elements, the important groups are: on the east, the Transyenissean, or East-Siberian, group with its subdivisions, and on the west, the West-Palearctic and West-Siberian groups. The demarcation line between them runs, roughly speaking, along the Yenissei River and then sweeps westward round the Altai highland. I have found that the Transyenissean species are strongly characterized, some presenting characters of relatively high antiquity; moreover, this group is intimately connected with the fauna of High Asia, many species reaching southward as far as the southern limits of the Palearctic; some of the Transyenissean forms are surely of southern origin. On the other hand, the fauna of western Siberia is in the main common with that of Europe; its distinctive forms are not numerous and are feebly characterized.

These facts find no explanation in recent conditions, but they are in full harmony with the paleogeography of these regions. The Transyenissean elements, strongly characterized, with features of antiquity and presenting relations to the fauna of High Asia—in spite of present differences of physical geography—correspond to the territory of that part of the Eurasian dry land which is known as the Angara continent. Western Siberia is a new dry land which emerged only at the beginning of the second half of the tertiary, and underwent considerable restriction of its area in later times. With this corresponds the paucity and prevailingly youthful character of its endemic forms. The transpalearctic distributions which prevail now are features of a later time, and for some of the animals involved traces of their Angaran, or western, origin may be found.

The fauna of High Asia is also strongly characterized, harmonizing well with the old age of this dry land. Prevalent in the fauna are biological types characteristic of desert, of mountain, and especially of dry highland. Sharp characteristics, and in some cases geographical distribution, of these biological types tell of a long duration of recent conditions. But at the same time the distribution of the fauna presents many gaps and many isolated remnants, these isolated colonies often having failed, as yet, to develop local characters. And all species which present an interrupted distribution are types characteristic of a more fertile landscape. These facts tell clearly of the extensive devastations to which the fauna has been recently subjected.

My interpretation is that present ecological types and corresponding environment are of an old origin, but that their present dominance is a new fact. I deny completely the theory of a large inland basin within the territory of High Asia in the Tertiary, and I consider the country as having presented the conditions of a continental climate, the land being more fertile, though with patches of desert here and there, and the rainfall more abundant than now.

M. Severtzoff and Professor Nassonov, after studying the distribution and structure of the wild sheep, have both come to the conclusion that this genus migrated from High Asia to America before the last glaciation; that in America they were driven south by the glaciation, that afterwards the American stock spread again to the north and gave origin to another group; and finally that this new group spread back into the Asian continent and occupied eastern Siberia, so that the present Siberian sheep stand, geographically and structurally, in no close relation with the sheep of High Asia. It may be mentioned that a similar course of migration is now admitted for certain human tribes.

The origin of species as revealed by vertebrate paleontology: A rejoinder to William Bateson: DR. HENRY FAIRFIELD OSBORN, American Museum of Natural History. This is a paleontological rejoinder to Bateson's Toronto address of 1921 in which he stated as the result of twenty-five years of genetic research: "Variations of many kinds, often considerable, we daily witness, but no origin of species. . . . That particular and essential bit of the theory of evolution which is concerned with the origin and nature of *species* remains utterly mysterious." Bateson and his school have been endeavoring to interpret the past by the present; the time has now come in biology to reverse this doctrine and *interpret the present by the past.* In paleontology we observe secular evolution and the adaptive action and reaction of the heredity germ to physical environment, to living environment, and to habit, under Osborn's principle of tetraplasy in form, tetrakinesis in function.

In biomechanical adaptation as observed in continuous genetic phyla of both invertebrate and vertebrate animals, the evolution of the germ-plasm can be continuously traced and is found to conform to nine chief biomechanical adaptations. Five of these principles have been discovered between the time of Aristotle and the present among living animals and confirmed among extinct animals. These are as follows: (1) Motion or function invariably precedes form; change of motion or function precedes change of form, just as in the inorganic world energy directs matter, not matter energy, as first observed by Aristotle. (2) The principle of compensation or economy of growth as defined by Aristotle and later by Geoffroy St. Hilaire. (3) The principle of biomechanical continuity discovered by the invertebrate paleontologist Waagen and confirmed by Osborn in the Titanothere Monograph. (4) The principle of germinal trend in a definite direction implied by Waagen and expressed by the Austrian paleontologist Neumayr in the term "mutationsrichtung." (5) The principle of acceleration and retardation observed by Von Baer in

embryology and elaborated in paleontology by Alpheus Hyatt. (6) The principle of allometry or change of proportion, the chief source of the origin of genera and of species, observed by Lamarck and Darwin and elaborated by Osborn in the Titanothere Monograph. (7) The principle of rectigradation of adaptive organs observed by Cope and elaborated by Osborn, confirmed in the primates, the horses, the rhinoceroses, the titanotheres, and the proboscideans; the second source of the origin of species. (8) The principle of experimental adaptation observed in the lower organisms in zoology, confirmed in the higher organisms like the primates and ungulates in paleontology; this principle accounts in part for the extraordinary divergence and diversity of the mammalian kingdom. (9) The principle of adaptive radiation, an elaboration by Osborn of Lamarck's *ébranchement* and of Darwin's "divergence." Under this principle as developed by Dollo and Osborn fall the biomechanical processes of homoplasy of Lamarck, of convergence, of alternating habitat elaborated by Dollo.

Four of these nine biomechanical principles are observed in paleontology only, which reveals the secular evolution of the germ-plasm, and could never be discovered in zoology, which reveals only the transitory fluctuations of saltations of the germ-plasm. These nine principles include exactly the modes by which new mutations, new species, new genera, new families and new orders of vertebrates arise. These principles of the origin of species are not theories or hypotheses, they are facts so clearly and repeatedly revealed that we can safely predict not only what the origin of the next species and genus will be like, but where this species or genus is likely to be found and in what stage of evolution it will be found. What really happens in the natural biomechanical origin of species is this: Whenever all the four energetic conditions of heredity, of environment, of biota, of habit or ontogeny, and the non-energetic condition of the struggle for existence (selection) are the same, we observe that there arises similar ascending mutations, species, genera, families, whether in France, in central Mongolia or in our own western territories. New similar species will arise at approximately, if not at precisely, the same rate, whether we observe them in France, Mongolia or our Rocky Mountain region.

Thirty-six years of intensive paleontological research have solved the question of the biomechanical origin of species and there is little to be added by discovery. Whether or not biophysical and biochemical specific characters arise according to similar principles remains to be demonstrated.

Method of determining and measuring the associative relations of species: PROFESSOR S. A. FORBES, University of Illinois. It is the object of this paper to describe and illustrate a method by which ecological affiliations and groupings may be accurately shown of species whose powers of locomotion and habits of migration make it impossible to study them by simple local inspection as may be done with stationary plants.

The materials necessary are numerous collections (as of fishes or insects), as nearly alike as possible, or observations (as of birds) so made and recorded as to show the numbers per square mile or other unit of area of each species in each habitat.

The method proposed is based on the obvious idea that the frequency with which species are found together in collections or by observations may be made a measure of their ecological affiliation, provided that joint occurrences due to mere chance or random distribution are distinguished and separated from those having an ecological origin. This requires for each pair of species studied a computation of the number of chance joint occurrences to be expected in each situation and a comparison with the number of actual joint occurrences observed. If these numbers are approximately equal no ecological affiliation of the species is indicated. If the actual is larger than the chance number, the difference is a measure of such affiliation and if it is smaller the species are being drawn apart into different environments.

By the use of the calculus of probabilities a coefficient of association is readily found for any two species which is a measure of the degree of ecological affiliation between them. This coefficient is expressed by the formula $\frac{ad}{bc}$; a being the total number of collections or observations made, b the number containing the more abundant, and c the less abundant species, and d the actual number of joint occurrences of the two species in the same collection or observation. Under the notation $\frac{bc}{a}$ = the number of chance joint occurrences to be expected and $\frac{d}{\frac{bc}{a}}$ or $\frac{ad}{bc}$ is the ratio of this number to the actual number of joint occurrences.

Such a coefficient taken by itself is unsatisfactory as a measure of ecological affinity unless the length of the series is known of which it is a part, and for this an expression for complete association of two species is necessary such that wherever one occurs the other may be expected also. This maximum possible number of joint occurrences in any case is the number of occurrences of the less abundant species (c), and $c - \frac{bc}{a}$ is an expression for the largest possible number of ecological joint occurrences in the case given, $d - \frac{bc}{a}$ represents the *actual* number of ecological joint occurrences and the quotient of the former divided by the latter is the ratio of the actual to the maximal number. The working formula consequently is not $\frac{ad}{bc}$ but $\frac{d - \frac{bc}{a}}{c - \frac{bc}{a}}$ by the use of which the ecological affiliation of pairs of species can be accurately expressed; and by a systematic tabulation of such ratios the affiliations of a group of species may be completely shown. Examples of such tables for collections of fishes and birds and of their use in forming ecological groups are given as illustrations of the method and of its product.